17

Energy Research Advisory Board
to the
United States Department of Energy
1000 Independence Avenue, S.W.
Washington, D.C. 20585
(202) 586-5444

August 28, 1989

TO: Members of the Cold Fusion Panel

Subject: Preparation for October 13 Meeting

Attached as a reminder is the Chairman's memorandum summarizing what needs to be done prior to our next meeting. In it John asks the Group Coordinators (Messrs. Fowler, Bard, Birnbaum and Schiffer) to have their working drafts completed by October 9 (which is a holiday). If possible, I would appreciate it if you could have your sections to me before that date so that I can mail copies to the other members. This would provide a few days prior to the meeting to study the material.

We are still planning to have the October 13 meeting at the Hilton Hotel at O'Hare airport, but the block of rooms will be reserved most probably at another motel at the airport which is more in line with cut allowable per diem. As soon as I know the details I will let you know.

William D. Woodard

Attachment

UNIVERSITY OF
ROCHESTER

COLLEGE OF ARTS AND SCIENCE
DEPARTMENT OF CHEMISTRY

John R. Huizenga
Tracy H. Harris Professor

TO: Members of Cold Fusion Panel

FROM: John R. Huizenga John R Huizenga

SUBJECT: Schedule of events and meetings to prepare our final report

DATE: July 20, 1989

Panel members present at the meeting in Washington on July 12 had a short discussion about the form and organization of our final report due in November, 1989. It was the opinion of members present on July 12 that the final report should be kept short (less than twice the length of the Interim Report) with back-up material going into appendices.

The following work schedule was adopted in order that we might have the final report ready by November 1, 1989. This schedule would allow the full Energy Research Advisory Board to review the report at its scheduled meeting on November 8 and 9. In order to meet the above schedule, the following dates and associated work schedules need to be adopted.

July 14, 1989 - Copy of our draft Interim Report to be sent from the DOE to all hosts of our site visits, all contacts (and Laboratory Directors) at national laboratories and selected individuals at industrial and university laboratories working on cold fusion. Each letter to accompany the Interim Report is to request a short summary of past and present research on cold fusion to be due at DOE by September 15.

September 15, 1989 - Above research summaries on cold fusion to be distributed by DOE to all panel members.

October 9, 1989 - Subpanels to have completed working drafts of their part of the final report, including appendices.

October 13, 1989 - Panel meeting at O'Hare Airport to work on first draft of final report. A group of rooms will be reserved at the O'Hare Hilton for the night of October 12, meeting scheduled for 8:00 am to 5:00 pm on Friday, October 13.

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October 30 and 31 - Panel meeting at DOE in Washington (Forrestal Building).

October 30:	8:00 am	Coffee
	8:30 am - 6:00 pm	with working lunch
October 31:	8:00 am	Coffee
	8:30 - 12:00 noon	

This one and one-half day meeting will be devoted to working on the final report.

The membership of the working groups as follows (the asterisk indicates the coordinator of each group):

Introduction:	Gavin, Fowler*, Huizenga and Koonin
Calorimetry:	Bard*, Faulkner, Happer, Miller and Wrighton
Materials:	Birnbaum*, Boudart, Dresselhaus, Nelson and Stein
Fusion Products:	Bigeleisen, Garwin, Hoffman, Koonin, Lipman and Schiffer*
Conclusions and Recommendations:	Bard, Birnbaum, Callis, Garwin, Huizenga* and Schiffer

In addition to rewriting the assigned section for the final report, each working group will need to decide on the content and write the appropriate appendices to back up their section. During the writing process, each group will no doubt be in a position to formulate draft statements that can be used in the section on conclusions and recommendations.

I greatly appreciate your cooperation and hard work so necessary in producing this report.

UNIVERSITY OF
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DEPARTMENT OF CHEMISTRY

John R. Huizenga
Tracy H. Harris Professor

To: Howard K. Birnbaum
Richard L. Garwin
William Happer, Jr.
Steven E. Koonin
David Nelson
John P. Schiffer

From: John R. Huizenga

Subject: Calculation of fusion rate of deuteron pairs in palladium

Enclosed is a preprint from Willis E. Lamb, Jr. that has been accepted for publication in the Proceedings of N.A.S. and a preprint of a "Comment" by R.H. Parmenter that has been sent to Phys. Rev. Lett.

The critical assumptions in the Lamb paper is the 10 conduction electrons per Pd atom, which results (by eq, 4) in a value of m*/m=3.4. His enhanced fusion rate is mainly due to this factor. Reducing the number of conduction electrons by a factor of 2, reduces m*/m to 1.07.

The final result given by eq. 28 is that $\lambda=2 \times 10^{-23}$ fusions per deuteron pair per sec (why does one give 5 significant figures to a number with an uncertainty of many orders of magnitude?). Jones et al. report a value of $\lambda=10^{-23}$ fusions per deuteron per sec. for Ti cathodes! However, Ti has an electronic configuration of $3d^2\ 4s^2$ and at most 4 conduction electrons. Hence, these two numbers should not be compared.

What is your reaction to this paper?

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Comment on "Exact Upper Bound on Barrier Penetration Probabilities in Many-Body Systems: Application to 'Cold Fusion'"

In their recent discussion of cold fusion, Leggett and Baym[1] made use of the relation

$$\lambda = A|\Psi(0)|^2 \quad , \tag{1}$$

where λ is the rate at which a pair of deuterons will fuse, and $|\Psi(0)|^2$ is the probability density of finding the two deuterons at $\vec{r} = 0$. These authors chose the value $A = 1.5 \times 10^{-16}$ cm^3/sec, and implied that this value is appropriate for <u>any</u> kind of barrier. In other words, they suggested that Eq. (1) involves only nuclear physics inside the barrier and is valid independent of the nature of the barrier. In order to show that this is <u>not</u> the case, we sketch a derivation[2] of Eq. (1) when A is defined as

$$A = \lim_{E \to 0} S(E)\,(\pi\alpha\mu c)^{-1} \quad , \tag{2}$$

S(E) is the nuclear S-factor, $\alpha = (e^2/\hbar c)$, μ = reduced mass.

Consider elastic scattering in an unscreened, repulsive Coulomb field, as analyzed by Davydov.[3] For the case $Z_1 = Z_2 = 1$, Davydov's Eq. (111.16) gives

$$|\psi^+(0)|^2 = (2\pi e^2/\hbar v^2)\, e^{-2\pi\eta} \quad , \tag{3}$$

$\psi^+(\vec{r})$ being the solution corresponding to an incident plane wave plus outgoing spherical wave. Using this, the Gamow factor can be written as

$$e^{-2\pi\eta} = (\pi\mu\alpha c)^{-1}\, E|\psi^+(0)|^2 \quad ,$$

where $E = \frac{1}{2}\mu v^2$. Thus, for small E, the cross section can be written

$$\sigma = E^{-1}\, S(E)\, e^{-2\pi\eta} = A\,|\psi^{+}(0)|^{2} \quad . \tag{4}$$

As can be seen by inspection of Eq. (3), $|\psi^{+}(0)|^{2}$ has the units of inverse velocity. Davydov states on p. 480 of ref. 3 that ψ^{+} is normalized to unit flux. Multiplying both sides of Eq. (4) by the incident flux ρv, one gets

$$\lambda = \rho v \sigma = A\,|\Psi(0)|^{2} \quad , \tag{5}$$

where $|\Psi(0)|^{2} = \rho v\,|\psi^{+}(0)|^{2}$. Choosing S(0) = 106 KeV barns, the combined S-factor corresponding to both fusion reactions d(d,n) ^{3}He and d(d,p) ^{3}H, I calculate $A = 1.478 \times 10^{-16}$ cm^{3}/sec.

Note that this derivation holds only for the unscreened Coulomb barrier, not any other kind of barrier. One might assume that the above value of A represents a maximum possible value obtainable with any kind of barrier. Such an assumption is logically inconsistent with the observation that if the strength of the Coulomb interaction, e^{2}, were cut in half, then A would be doubled in size [because of the presence of the fine-structure constant in Eq. (2)]. In effect, much larger values of A have been obtained by Willis Lamb and the writer[4] in our analysis of cold fusion.

R. H. Parmenter

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1. A. J. Leggett and G. Baym, Phys. Rev. Lett. 63, 191 (1989).

2. The quantity A was apparently first discussed by J. D. Jackson, Phys. Rev. 106, 330 (1957), without giving a general derivation or explicit expression for its form.

3. A. S. Davydov, "Quantum Mechanics," translated by D. ter Haar (Pergamon Press), Sec. 111.

4. R. H. Parmenter and Willis E. Lamb, Jr., Proc. Nat. Acad. Sci., in press.

Cold Fusion in Metals

(Physics)

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Contributed by Willis E. Lamb, Jr.

Abbreviations: E_F, Fermi energy; n_0, conduction electron density; a_0, Bohr radius; E_0, Hartree energy; λ_D, Debye screening length; ω_0, characteristic frequency; TFM, Thomas-Fermi-Mott.

Abstract

A simple model of a metal containing deuterons is considered. The example of palladium is treated in detail. It is shown that the effect of screening of Coulomb fields by conduction electrons is sufficient to allow deuteron pairs to fuse at rates comparable with those reported by Jones et al. in experiments at Brigham Young U- .sity.

The recent reports of cold fusion in metals, both at high levels[1] and at much lower levels,[2] have stimulated great interest and considerable skepticism.[3,4] In this note, we propose a simple model which suggests that cold fusion, at least at the lower levels, is quite possible. The model makes use of the fact that screening of Coulomb fields in a metal, a collective effect involving many conduction electrons, may be much more effective than the screening of Coulomb fields by valence electrons in a molecule.

We consider the possibility of cold fusion of deuterons in palladium metal, which has a face-centered-cubic lattice with cube edge L = 3.879 A. Any four nearest-neighbor Pd atoms form the vertices of a regular tetrahedron whose edge length is $\frac{1}{2}\sqrt{2}$ L = 2.743 A. The center of the tetrahedron is $\frac{1}{12}\sqrt{3}$ L = 0.5599 A distant from any of its faces, and $\frac{1}{4}\sqrt{3}$ L = 1.6797 A from any vertex. There are two such cages for every Pd atom. In addition, there is a larger octahedral cage for every Pd atom. We will first consider the possibility of deuterons being trapped in such a cage.

In palladium metal, the outermost ten 4d electrons furnish the metallic binding. We make the approximation of treating all ten such electrons per atom on the same footing, considering them to be conduction electrons with some effective mass. According to the jellium model of a metal,[5] the Fermi energy E_F is related to the longitudinal velocity of sound s by the relation

$$E_F = \tfrac{3}{2} Ms^2 \quad , \tag{1}$$

where M is the atomic mass per conduction electron. For s, we take a weighted rms average of the velocities[6] associated with the three symmetry directions, making use of the fact that there are three (100) directions, six (110) directions, and four (111) directions. Thus

$$s = \left\{\rho^{-1}\left[c_{11} + \tfrac{14}{13}(c_{12} + 2c_{44})\right]\right\}^{1/2} = 7.0781\times10^5 \text{ cm/sec} \quad ,$$

where $\rho = 11.40$ g/cm^3 is the mass density and the c_{ij} are the elastic constants. We now have

$$E_F = 8.3473 \text{ eV} \quad . \tag{2}$$

It might be objected that ten conduction electrons per atom is too many, two or four might be more appropriate, these being the two accepted valences of Pd. But with these smaller numbers, we would obtain unreasonably large values of E_F, especially for a band built primarily out of d electrons. MacDonald et al.[7] have calculated a 4d bandwidth in Pd of 6.26 eV.

The density of conduction electrons is

$$n_0 = 4\times10/(3.879)^3 = 0.68533 \text{ A}^{-3} \quad . \tag{3}$$

In the free-electron model of a metal, $E_F = (p_F^2/2m^*)$, $n_0 = (3\pi^2)^{-1}\ (p_F/\hbar)^3$, p_F being the Fermi momentum We then have

$$(m^*/m) = \tfrac{1}{2}\ (3\pi^2)^{2/3}\ (n_0 a_0^3)^{2/3}\ (E_0/E_F) = 3.3958 \quad . \tag{4}$$

Here m^* is the effective mass, m is the electronic mass, a_0 is the Bohr radius, and the Hartree energy E_0 is

$$E_0 = (\hbar^2/ma_0^2) = (e^2/a_0) = 27.21 \text{ eV} \quad .$$

The Debye screening length in a metal, λ_D, obtained by linearizing the Thomas-Fermi method,[8] is

$$\lambda_D = (E_F/6\pi n_0 e^2)^{1/2} = 0.21183 \text{ A} \quad . \tag{5}$$

A deuteron will experience a potential energy minimum at the center of a trap.

Assuming a uniform conduction electron density n_0, the potential energy in the vicinity of the center will be $U_1 = \frac{1}{2} \kappa r^2$, where the effective spring constant

$$\kappa = \tfrac{4}{3} \pi n_0 e^2 = \tfrac{2}{3} (E_F/\lambda_D^2) = 41.441 \text{ eV/A}^2 \quad . \tag{6}$$

The deuteron will oscillate with a characteristic frequency

$$\omega_0 = (\kappa/m_d)^{1/2} = 4.4560 \times 10^{14} \text{ sec}^{-1} \quad ,$$

and the corresponding ground-state classical turning radius will be

$$\mathcal{R} = (\hbar\omega_0/\kappa)^{1/2} = 0.08413 \text{ A} \quad .$$

We have been considering a bare deuteron in the trap. What about a neutral deuterium atom? In order for an electron to bind to the deuteron, it is necessary[9] that $2(\lambda_D/a_0) > 1.6799$, clearly not satisfied here where $(\lambda_D/a_0) = 0.3998$.

We are particularly interested in the situation where there are two deuterons in a trap. The Hamiltonian for this problem separates in terms of the center-of-mass coordinate $\vec{R} = \frac{1}{2} (\vec{r}_1 + \vec{r}_2)$ and the relative coordinate $\vec{r} = (\vec{r}_1 - \vec{r}_2)$. We have

$$H = H_R + H_r \quad , \qquad H_R = (\hbar^2/4m_d)\nabla_R^2 + \kappa R^2 \quad , \tag{7}$$

$$H_r = (\hbar^2/m_d)\nabla_r^2 + \tfrac{1}{4} \kappa r^2 + V_{eff}(r) \quad , \tag{8}$$

where $V_{eff}(r)$ is the effective interaction between two deuterons in the presence of screening conduction electrons. The major problem of this paper is to calculate V_{eff}.

First we consider the additional potential resulting from the presence of a single deuteron in the metal. To this end, we make use of a modification of the Thomas-Fermi

equation, which Mott[10] used to treat the potential resulting from a localized impurity in a metal.[11] This equation, which we refer to as the TFM equation, is

$$\left.\begin{aligned} \nabla^2 V(\vec{r}) &= -4\pi e^2\left[n(\vec{r}) - n_0\right] \quad , \\ n(\vec{r}) &= (8\pi/3h^3)\,(2m^*)^{3/2}\left[E_F - V(\vec{r})\right]^{3/2} \quad , \\ n_0 &= (8\pi/3h^3)\,(2m^* E_F)^{3/2} \quad . \end{aligned}\right\} \tag{9}$$

In the case we are considering, of spherical symmetry, it is convenient to express distances and energies in Thomas-Fermi units, distance measured in units of

$$a_F = \tfrac{1}{2}\left[\tfrac{3}{4}\pi\right]^{2/3}(m/m^*)a_0 = 0.88534(m/m^*)a_0$$

$$= 0.13797\ \text{A} \quad ,$$

energy in units of

$$(a_0/a_F)E_0 = 104.3671\ \text{eV} \quad .$$

We write

$$V(\vec{r}) = -e^2 r^{-1}\phi(x) \ , \quad x \equiv (r/a_F) \quad . \tag{10}$$

The TFM equation becomes

$$(d/dx)^2\phi = x^{-1/2}(\phi + Dx)^{3/2} - D^{3/2}x \quad , \tag{11}$$

where $D = 0.079980$ is the Fermi energy expressed in these new energy units.

Starting at $x = 0$ and subject to the boundary conditions $\phi(0) = 1$, $\phi'(0) = -m$, this equation was integrated numerically out to large values of x. The constant m was chosen such that $\phi(x)$ vanishes as $x \to \infty$. A variant of the Noumerov method[12] for integrating linear second-order differential equations was found to be quite efficient. At large

distances, $\phi(x)$ becomes proportional to $\exp[-(a_F/\lambda_D)x]$. It is important to point out that the proportionality factor is less than one. At large distances, it appears that a charge smaller than unit charge is being screened out by the conduction electrons.

It is convenient for what follows that we have an accurate analytic approximation to $\phi(x)$. With an error of less than 0.2 percent, we can approximate $\phi(x)$ by the expression

$$\phi_{app}(x) = \sum_{i=1}^{5} A_i e^{-\alpha_i r} \quad , \tag{12}$$

where A_i and α_i are listed in Table I. This expression satisfies boundary conditions at $x = 0$ and $x \to \infty$.

Thus far, we have considered a single deuteron. Now consider a pair of deuterons. We make the approximation that the one-electron potential associated with the pair is simply the sum of the potentials associated with each deuteron by itself, i.e.,

$$\left.\begin{aligned} V(\vec{r}) &= V_1(\vec{r}) + V_1(\vec{r} - \vec{R}) \quad , \\ V_1(\vec{r}) &= -r^{-1}\,\phi(r) \quad . \end{aligned}\right\} \tag{13}$$

(We are still measuring distance and energy in Thomas-Fermi units.) Here we have one deuteron at $\vec{r} = 0$, the other at $\vec{r} = \vec{R}$. This approximation, if anything, will underestimate the screening when the two deuterons are close. We will return to this point later. The electrostatic interaction energy of the two screened deuterons can be broken into three pieces. The Coulomb interaction energy of the screening electrons is

$$\tfrac{1}{2} \int \{V(\vec{r}) + r^{-1} + |\vec{r} - \vec{R}|^{-1}\}\,\{n(\vec{r}) - n_0\} d^3r \quad ,$$

where

$$n(\vec{r}) - n_0 = -(4\pi)^{-1} \nabla^2 V(\vec{r}) \quad .$$

The factor of $\frac{1}{2}$ is to correct for double counting. The Coulomb interaction between deuteron nuclei and screening electrons is

$$-\int \{r^{-1} + |\vec{r} - \vec{R}|^{-1}\} \{n(\vec{r}) - n_0\} d^3r \quad .$$

The Coulomb interaction between the deuteron nuclei is $+R^{-1}$. Thus the total electrostatic interaction energy is

$$V_E(R) = + R^{-1} - I_1 - I_2 \quad , \tag{14}$$

where, after some algebra,

$$I_1 \equiv (4\pi)^{-1} \int r^{-2} [1 + \phi(r)]\phi''(r) d^3r$$

$$= \int_0^\infty dr[1 + \phi(r)]\phi''(r) \quad , \tag{15}$$

$$I_2 \equiv (4\pi)^{-1} \int (rr')^{-1} [1 + \phi(r)]\phi''(r') d^3r$$

$$= (2R)^{-1} \int_0^\infty dr[1 + \phi(r)] [\phi'(r + R) - \phi'(r - R)] \quad . \tag{16}$$

(We are using the notation $\vec{r}' \equiv \vec{r} - \vec{R}$) I_1, being a constant independent of R, may be dropped by redefining the zero of energy. Making use of the analytic expression for $\phi(r)$, we get

$$V_E(R) = + R^{-1} - I_2 = R^{-1}\,\bar{\phi}(R) \quad , \tag{17}$$

$$\bar{\phi}(R) \equiv \sum_{i=1}^{5} (C_i - D_i R)e^{-\alpha_i R} \quad , \tag{18}$$

$$C_i \equiv A_i(1 - \tfrac{1}{2}\,B_i) \quad , \tag{19}$$

$$B_i \equiv \sum_{j\neq i}^{5} 2A_j(\alpha_j^2 + \alpha_i^2)\,(\alpha_j^2 - \alpha_i^2)^{-1} \quad , \tag{20}$$

$$D_i \equiv \tfrac{1}{2}\,\alpha_i A_i^2 \quad . \tag{21}$$

Note that $\Sigma_i\ C_i = 1$. The values of C_i and D_i are listed in Table I. It can be seen that $C_1 < A_1$, whereas $C_i > A_i$ for $i \geq 2$. This means that $V_E(r)$ is much more sharply peaked at the origin than is $V_1(r)$; i.e., the barrier of the former is narrower. $V_E(r)$ is the desired $V_{eff}(r)$ of Eq. (8). It may be considered the interaction between two identical composite particles, our composite particle being a bare deuteron plus the associated screening cloud of conduction electrons.

The total effective potential for a deuteron pair in a trap, expressed in Thomas-Fermi units, is

$$U(x) = x^{-1}\,\bar{\phi}(x) + \kappa' x^2 \quad , \tag{22}$$

where $\kappa' \equiv 0.0018896$. This has a minimum at $x = 2.7331$. The characteristic frequency of zero-point oscillations about this minimum is $\omega_0 = 1.1545\times10^{15}$ sec^{-1}, and the energy of this state

$$E_T = U_{min} + \tfrac{1}{2}\,\hbar\omega_0 = 0.018210 \quad .$$

The inner classical turning point of this state, x_c, is close to $x = 2.27$.

The probability per unit time that two deuterons in a trap will fuse is given by

$$\lambda = (\omega_0/2\pi)\ (\sigma/4\pi R_n^2) \quad , \tag{23}$$

where σ is the cross section for fusion and R_n is the range of the nuclear interaction.[13] We take $R_n = 3.22$ F, twice the experimentally observed rms radius of an alpha particle. We have

$$\sigma = E^{-1}\ S(E) e^{-2\pi\eta} \quad , \tag{24}$$

where $\exp(-2\pi\eta)$ is the Gamow penetration factor, and S(E) is the nuclear S-factor, a slowly varying function of energy E. Here $E = \frac{1}{2}\hbar\omega_0$. At low energies, for d-d fusion, we take[14] $S(E) = S(O) = 1.06\times10^2$ keV barn. Thus

$$\lambda = \hbar^{-1} S(O)\ (2\pi R_n)^{-2}\ e^{-2\pi\eta} = 3.9339\times10^{19}\ e^{-2\pi\eta}\ \text{sec}^{-1} \quad . \tag{25}$$

We have

$$\left.\begin{aligned} 2\pi\eta &= K\int_0^{x_c}\left[U(x) - E_T\right]^{1/2} dx \quad , \\ K &= 2\left[(m_d/m)\ (a_F/a)\right]^{1/2} = 61.868 \quad , \end{aligned}\right\} \tag{26}$$

we find $2\pi\eta = 104.796$, so that

$$\lambda = 1.2089\times10^{-26}\ \text{sec}^{-1} \quad . \tag{27}$$

As already mentioned, the approximation embodied in Eq. (22) will tend to underestimate the screening when the two deuterons are close. Electrons much further away

from the origin than the two deuterons will find themselves in the screened field of an impurity of charge Z = 2. This suggests the alternative approach of letting $V_1(\vec{r})$ be one-half the solution to the TFM equation for a single alpha particle, rather than the solution for a single deuteron. Such an approximation, if anything, probably slightly overestimates the screening when the two deuterons are well separated. However, it is the behavior when the deuterons are close together that plays a crucial role in determining the size of the Gamow factor.

We have solved Eq. (11) for the case Z = 2, and have analytically fitted one-half of the resultant $\phi(x)$ in the same manner as before. The various parameters are listed in Table II. The resultant U(x) has a minimum at x = 2.5381, the classical turning point is close to x_c = 2.08, the characteristic frequency is $\omega_0 = 1.1638\times10^{15}$ sec^{-1}, and the energy is E_T = 0.016378. We find $2\pi\eta$ = 97.232, so that the pair fusion rate is

$$\lambda = 2.3313\times10^{-23} \text{ sec}^{-1} \quad . \tag{28}$$

This number is in good agreement with the observations of Jones et al.,[2] although it should be mentioned that they were using titanium, not palladium.

We recognize that, rather than using either of the two approximations involving Eq. (13), it would be preferable to solve Eq. (9) directly for the case of two deuterons separated by a distance R, and from the results calculate $V_E(R)$. This is under investigation at the present time. The numerical difficulties involved are considerable.

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Table I. Parameters for the analytic representations of ϕ and $\bar{\phi}$; Z = 1 case.

i	α_i	A_i	C_i	D_i
1	0.651324	0.6485306	0.3521383	0.1369708
2	1.261690	0.0924173	0.1542122	0.0053880
3	2.234930	0.2000000	0.3742368	0.0446986
4	5.602250	0.0459964	0.0933864	0.0059263
5	29.910950	0.0130557	0.0260263	0.0025492

Table II. Parameters for the analytic representations of ϕ and $\bar{\phi}$; Z = 2 case.

i	α_i	A_i	C_i	D_i
1	0.651324	0.5315000	0.2166725	0.0919970
2	1.547610	0.2470066	0.3481617	0.0472116
3	2.922640	0.1602994	0.3122658	0.0375500
4	7.906130	0.0546916	0.1099179	0.0118243
5	61.319420	0.0065024	0.0129821	0.0012963

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Dear E632 and WA84 Colleagues, 25 August 1989.

COLD FUSION NEWS No. 19

Cold Fusion Heats Up! New Institutes Founded.

Notes on LEP

1. SUMMARY

The last week has been rather wild and quite fascinating. It might be thought that the recommendation of the Interim report of the DOE Panel on Cold Fusion "against any significant expenditures to establish cold fusion research centres" might have some effect. But the State of Utah has now set up the National Cold Fusion Research Institute after its Advisory Board appeared to have heard only positive testimony. Strong additional funding is expected from industry. Neighbouring BYU does not seem to have been involved, but they have now established their own Fusion Research Centre!

The menace of Japan taking over a great American discovery has been brandished. Now it is reported that a Japanese Institute of Fusion Science is being organised. Also in India a five foot tall cathode is being built which would be bigger than any other.

Then yesterday there was quite a bit of excitement in Utah when Mike Salamon gave a press conference to announce their results (see No.18), that their neutrons and gamma counters see no fusion from the cells in Dr. Pons's lab. The response of Dr. Pons was breath-taking (section 6).

Much of the justification of the Utah State Advisory Board came from the positive results obtained in Florida and at Texas A&M. However the Florida results are contested by a group upstairs(section 7.1) and the lack of checks for the Texas results are discussed in Section 7.2. Finally all positive and negative results are briefly summarised in Section 7.5

Comments from Utah (and elsewhere) on the DOE panel report are fine examples of contemporary politics and worth savouring (section 8.3).

2. STARTING UP OF NATIONAL COLD FUSION RESEARCH INSTITUTE

2.1 Introduction

A major new Research Institute has been set up which is intended to be National, not for the State of Utah only. The funding is expected to be national also. How this came about despite the recommendations of a federal panel, is a classic case worthy of study by students of Science Politics and of Life. The essential seems to be to reinforce the positive and exclude the negative - peer review is excluded.

2.2 Procedings and Vote of Advisory Board

The Utah State Advisory board on Energy/Fusion held hearings to establish whether the original claims of Profs Fleischmann and Pons were confirmed. All accounts agree that only positive results were presented. As they appeared impressive, it is only fair to the board members if we give them.

B. Stanley Pons told the Board that he had used "Cold Fusion" to boil water and suggested that household uses for his purported discovery were in the offing. In his experiment a "boiler" the size of a thermos emmitted 15 to 20 times the amount of heat being put into it -- a reaction that cannot be explained by normal chemical reactions, Dr. Pons said (Rayleigh Times and Observer, July 12). On the other hand it was said by GE that Drs. Pons and Fleischmann have admitted that their early data on gamma ray spectroscopy was flawed. It was said the results were " inappropriately homogenised" (SLC Tribune).

A report from the U. to the state Energy/Fusion Advisory Council was leaked to the Salt Lake City Tribune. It lists the "silent" organisations that have obtained confirmation of Fleischmann and Pons's work but have not published.

It said that scientists at the General Electric Company's "research facility at Schenectady, NY had reproduced the experiment and have 'obtained excess energy at about the 15% level'. The report also says that GE 'after long and careful study..... concludes that the basic calorimetric theory of Pons and Fleischmann is correct and shows excess energy'. GE refused to confirm the report. 'We're not confirming or disclaiming heat', said Peter Van Avery, GE's manager of communications [an interesting title - does it mean manager of telecommunications or of public relations? - my comments are in square brackets] at Schenectady. 'We're not making any claims at all, but the research is continuing.'"

" A scientist from GE also examined the research of (U College of Mines Dean) Dr. Wadsworth, the report said. 'The GE thermodynamist concluded that those data cannot be explained by any error in measurement, nor any chemical phenomenon'"

The Deseret News later said that "The GE scientists also have reported finding significant levels of tritium in the electrolyte, but no signs of helium 4 in the bar".

"The report also said Edmund Storms and Carol Talcott of Los Alamos National Laboratory have electrochemical cells that produced excess heat 'after being coated with a lithium-palladium alloy'" [probably it was the electrodes that were coated].

"Drs. Storms and Talcott had earlier reported the production of tritium, a fusion by-product, in the same cells. Both researchers were unavailable for comment Tuesday".

"Charles D. Scott of Oak Ridge National Lab. has one cell that is showing 'small excess of heat(about 8%)', the report said. Dr. Scott was not in his office Tuesday afternoon, but Oak Ridge spokesman Ed Aebischer said 'He has apparently seen some evidence of heat production'. Mr. Aebischer added that the results were preliminary".

"More confirmation of heat was attributed to Michael Mackubre, a researcher at the Stanford Research Institute in Menlo Park, California. 'He has had several incidents of extended excess heating', the report said"

"The report said Dr. Mackubre used closed system calorimetry. Some critics of the U. research have said that a closed system, where none of the evolving gases is allowed to escape the cell, is more accurate than the open system used by Dr. Pons and the others who have reported the heat."

"Dr. Mackubre was unavailable but the results were acknowledged by a representative of for the Electrical Power Research Institute, which is funding the research."

"'That is correct', said John Maulbetsch, senior adviser in the office of exploratory research at EPRI. EPRI is a non-profit research consortium funded by electrical utility companies. 'There have been some indications of heat, but they have been sporadic'."

"And another scientist at Case Western Reserve University reported 'low levels of excess energy'. The researcher was identified only as E. Yeager."

"The report also made reference to instances of tritium production by Thomas Furtek at the University of Colorado. Tritium has been reported by a number of laboratories, but it has come in levels far below the excess heat."

The U student newspaper, the Summer Chronicle, reports some of the testimony of Drs Pons and Huggins. "Pons said one cell he has been experimenting with produced as much as 22 Watts of continuous power. 'Any reasonable engineer would be able to upscale (the experiment) for commercial use', Pons said. For example, the first commercial product will most likely be a typical water heater for a family home."

He said they would be submitting two papers in the next few months [elsewhere it is suggested that the first will be in September but in talking to Martin Fleischmann yesterday, he described to me so many calculations and other work that he would like in the paper, that it could take longer to write and to review]. "The first paper will detail the calorimetric measurements of the experiment and will present error limits for each calculation." It will perhaps be submitted to the Journal of Electroanalytical Chemistry [it will be interesting to see if it is reviewed the same way as their first paper which was strongly criticised - for example it is the first paper on calorimetry that I have seen that did not give a single temperature]. "The second will analyze the compound tritium which has been detected in small amounts. Pons said tritium is not found naturally and is only present after a nuclear reaction."

"Huggins said his fusion experiments have also produced excess heat and critics of the U experiment are not performing the thermal measurements correctly. 'At Stanford we have four sets of cells operating and each one has given off heat. But the heat from the experiments is giving very sporadic results. This shows that there are still a lot of questions to be answered'

Huggins said."

On 21 July the Fusion/Energy Advisory Council voted unanimously to accept the work of B. Stanley Pons and Martin Fleischmann as confirmed. In view of the evidence presented above to the Council and the absence of any report of all the other experiments which found no effect, it was reasonable for the council to vote this way. However the Salt Lake Tribune noted that "it withheld approval of budget specifics because of concern over administrative costs".

"The fusion council's executive committee expedited its approval.... upon learning that the fusion race was gaining speed - particularly at the University of Florida where the most favourable confirmation of the U experiment was recently announced. Southern scientists said Gamma rays, tritium, and excess heat sufficient to light a light bulb, have been produced" (Deseret News).

On 7 August the state council voted to release $4.5 million for research into cold fusion and this allows the funding of a National Cold Fusion Institute for Research. The vote was 7 to 1 plus one abstention - he was Wilford Hansen, the only physicist on the panel, who had caused some controversy by staying on the
the panel although at the last moment he had applied for $750 000 of this money for his research. The negative vote was believed to be Dr. Karen Morse who is Dr. Hansen's Provost at USU who was particularly worried about the high administative costs. These two are the only scientists on the panel.

State officials said that this money represents one-third of the State's annual investment in economic development (Deseret News).

U officials said the initial staff will be composed of U faculty members supplemented by interested faculty from Utah State University, BYU, and visiting researchers from other universities and industrial labs. A permanent staff will be developed as quickly as funding levels and the recruiting process permits (Deseret News).

Someone who has studied the State bill to fund the research says that it is carefully written so that funding could be given even though the effect is not fusion.

2.3 Funding of The National Institute

A 25 000 square foot facility has been taken over in the University's Research Park and apparently the first employees were hired some time ago. There is a five year funding plan. Of the $4.5 million voted by the Utah State legislature, between $2.8 million (75% of the Institute's budget) will be spent the first year and the remaining $1.7 m (37%) in the following year. This is regarded as seed money and it is expected that a further $20 million will be forthcoming from industry and government sources. Dr. Pons has said that he will not be joining the Institute as he wishes to concentrate on experiments he should do and wants to do. Some Council members were worried that Drs Pons and Fleischmann would not be adequately funded but Dr. Brophy said "Dr. Pons will get whatever he wants" (Deseret News). He has received funding from the Office of Naval Research (SL Tribune).

The Electrical Power Research Institute, EPRI, is a group sponsored by many members of the electrical power industry. Its leaders and members appear to be believers in the production of excess heat, though they are cautious about other claims. While it is expected that government agencies will fund, this will take some time; the EPRI will fund fairly rapidly, in a few months. The local power utility, Utah Power and Light, is not a member of EPRI, though they follow cold fusion - but they look at ALL the data, both positive and negative.

The Deseret News of 8 August reported that GE has signed a financial agreement to develop fusion research. U. President Chase Peterson said that GE's investment would be "sizable". Dr. James Brophy told the Council that additional corporate funding, likely to include Westinghouse, is being sought and eventually funding from the DOE is anticipated. In this connection, Robert L. Park of the APS reports that Rep. Wayne Owens (R-UT) whose district includes the U, recently sent a "Dear Colleague" letter to fellow members of Congress, urging them to keep an open mind on cold fusion in spite of the negative

report of the DOE cold fusion panel.
It is expected that ONR and NSF will fund but mildly.

2.4 EPRI Meeting to discuss positive results.

Yet another meeting has been held at which results and experimenters were selected. Only positive results on cold fusion were given. Dr. Bockris of Texas A&M told me that it was a most impressive meeting. The results from Florida are the most advanced in respect of the fact that they have heat, tritium, neutrons and gamma rays.

He considers there are distinct "types of heat emission;
(1) For most of the time the electrodes do nothing
(2) For maybe a quarter of the time.....they emit a low-level heat .... 10 to 15% excess heat.
(3) What is really exciting is that from time to time the electrodes emit heat which is often 3 to 4 times and sometimes 7 to 8 times, the energy being put in. These bursts last anywhere between 10 minutes to 90 hours. Many of them last 10 to 15 hours. They stop as suddenly as they have begun."

For tritium, he says that 7 or 8 labs have it now though 2 government labs will not announce until they get reproducibility. At Texas they have tried 12 electrodes now and get tritium in large amounts from all of them, but the curious thing is that you never know when it will start - between 7 days and 10 weeks.

The argument as to whether it is a volume or a surface effect continues. Prof Bockris considers that there "are two parties now;
(a) The Fleischmann-Pons party which thinks everything is compressed inside the electrode
(b) The Bockris party which believes that things happen because of very high fields at dendritic tips on the surface."

It would be good to have some solid experimental evidence to justify these two opinions.

Hear rumours that there may be other meetings organised at which only positive results and not all results, will be presented.

3. STARTING UP OF BYU FUSION RESEARCH CENTRE

Not everyone in Utah agreed with the setting up of a National Fusion Research Institute in Utah. When Stephen Jones of Brigham Young University was interviewed for local news on channel 2, he said that he "respectfully" disagreed with the findings of the committee. "We have input that would be useful to the committee", he said. However the Y News, a paper of BYU, on 11 August wrote that "A Center for Cold Nuclear Fusion Studies
has been established on campus with B. Kent Harrison, professor of physics as director. Stan L. Albrecht BYU academic vice-President and associate provost said the centre will conduct research on a wide range of fusion-related projects, expanding on the work of Stephen E. Jones and his associates who have been conducting cold nuclear fusion research at BYU since 1986."

"Jones, an associate professor of physics and Douglas N. Bunion, a professor of chemical engineering and chairman of that department, will serve as Associate Directors of the center."

"Harrison said the centre will facilitate the scholarly exchange of information in peer-reviewed journals and at professional meetings and will correlate fusion research at BYU and with other organisations. He said the centre will also attract funding in support of research and develop commercial applications for cold nuclear fusion."

It may be seen that these two centres in Utah have quite different styles.

4. STARTING UP OF A JAPANESE INSTITUTE FOR FUSION RESEARCH
+ 5. BUILDING OF A GIANT FUSION CELL IN INDIA

Have heard some reports of a centre being set up in Japan. The fullest account is in the

Salt Lake Tribune of August 24;

"The editor of an Asian science and technology journal said Wednesday that scientists in Japan have organized an Institute of Fusion Science and are rapidly moving ahead in cold fusion.

"Japan is the most organized of all the countries," said Ramtanu Maitra, editor of Fusion Asia, a journal of energy and other technology issues published in New Delhi.

Mr. Maitra was in Utah to see fusion scientists at the University of Utah and Brigham Young University. He met with BYU physicist Paul Palmer Tuesday and with U. of U. College of Mines Dean Milton Wadsworth and National Cold Fusion Institute Director, Hugo Rossi, Wednesday.

Mr. Maitra has a master's degree in nuclear physics from the State University of New York at Stonybrook, but he said he came to Utah as a scientific journalist, not a nuclear scientist. Mr. Maitra said the institute was set up Aug. 1, and some 80 scientists will join under the leadership of Hideo Ikegami, a respected scientist.

He said the Japanese are very cautious and would not embark on such a thing unless it was worth pursuing. "They have found something. It's very clear."

University of Utah officials, in their bid for fusion funding, have repeatedly raised the specter of Japanese scientists using an organized effort to commercially exploit cold fusion before the United States.

He also said the Japanese tend to take a long-term approach to their research. "They won't get very euphoric, but they won't get very discouraged either....You have to get out of the mind-set that if it doesn't happen fast, it doesn't happen at all.

"We have to remember that Japan has an advanced hot-fusion research base," Mr. Maitra said. "This is not something totally new for them."

The Japanese have been closed-mouthed about fusion, he said. It's very difficult to get information...

But he believes they will eventually be willing to publish more on fusion than their U.S. counterparts, who tend to classify such research for national security or patent reasons.

Mr. Maitra also said his own country, India, has stepped up its cold-fusion efforts at the country's nuclear research centers, including the Trombay Nuclear Research Center. Trombay, about 20 miles from Bombay, has 12,000 scientists [surely an error - it should be "staff" not "scientists"], he said.

One Indian group has build a five-foot-tall cathode for a cold-fusion cell, which far exceeds anything built in the United States. "They wouldn't have gone for it if they were't seeing anything significant," he said.

He said Asian scientists are acutely aware that cold-fusion research was launched in Utah, and several of them expressed envy that he was visiting here."

Have minimal confirmation of what Mr. Maitra has said and would welcome any completely independent confirmation. In the early days there were reports of work on cold fusion in Japan and Tokyo and Hokkaido Universities were reported as confirming some effects, but since then there have been remarkably few reports. Maybe as Sherlock Holmes said in the Hound of the Baskervilles, the real question is why the hound did NOT bark.

In India three labs have reported results which could be considered positive confirmation. Dr. P. K. Iyengar, Director of the Bhabba Atomic Research Centre at Trombay, Bombay, has sent me a paper "Cold Fusion in BARC Experiments" where he reviews a series of positive results obtained by groups at his institute. He says that full papers are being prepared and one looks forward to receiving them.

6. UTAH REPORTS NO NEUTRONS OR GAMMAS FROM PONS'S CELLS

In the previous note, No. 18, it was explained that while several of Pons's cells were running on a table in his lab, Mike Salamon, Ed Wrenn and Haven Bergeson and Collaborators were operating neutron and gamma counters underneath the same table. No fusion

products were detected.

On 24 August Drs Salamon and Bergeson went public with their results which caused considerable local interest. They have written a paper for Nature. Their main result are;

a) the upper limit for neutrons corresponds to less than one billionth of a Watt from the reaction d + d ---> 3He + n
b) the upper limit for gammas corresponds to less than one billionth of a Watt from the reaction d + d ---> 4He + gamma
c) the upper limit for internal conversion electrons is about one billionth of a Watt - these would be the E2 nuclear gammas from Coulomb excitation of the even-even Pd isotopes by the 3 MeV protons produced in the reaction d + d ---> t + p

These would seem to cover the main fusion reactions.

These results would appear to exclude the helium internal conversion theory of Chaves Walling and John Simmons. And what is important today, be in contradiction with the results reported on tritium production. It would be good if it were possible for a group finding tritium were to co-operate with the Utah group to make simultaneous measurements as a single measurement of a surprising result needs confirmation by a simultaneous measurement of a different nature, to be generally acceptable.

Neither physicist was willing to dismiss Pons and Fleischmann's results but they said it would require a mechanism unknown in nuclear physics.

According to a previous agreement they showed their paper first to prof. Pons. His answer was very simple - in the six weeks in May and June when they were running, there had been no heat excursions. The Salt Lake Tribune quoted;

"'I'm not at all surprised by their results' said Dr. Pons, who said the cells they were monitoring were running at barely detectable levels. Dr. Pons said the cells had none of the heat 'bursts' that have been reported. These bursts have produced up to 50 times the energy supplied by the cells battery, Dr. Pons has reported, but they have been extremely elusive."

"'We have purposely kept the power amounts low on these cells', Dr. Pons said, explaining that he and colleague Martin Fleischann are trying to 'lower our error bars' in their heat detection."

All this is rather strange. In his previous report in No. 18, it was written "During this period we were informed at least twice that there was at least one "active" cell; during one of these times, the D20 electrolyte was personally observed to be boiling. Our discussion focusses on the episode, which lasted approximately two hours until the cell was turned off under Prof. Pons's instructions -- this was to avoid a catastrophic event". Also Dr.Pons's group tried to generate cell activity for them by changing the current suddenly from 100 mA to 600 mA in the most promising cell (the one that boiled later). There seems a contradiction here. However it could easily be resolved by looking at the original log books kept by Pons's group. From past history, it is possible that the reply will be given that the lawyers will not allow it. This would be a surprising reply since Dr. Salamon has measured one billionth of a Watt which could be of no practical importance to a lawyer. However the argument might still be employed, so it is to be hoped that the pages will copied and stored separately and carefully to avoid any unfortunate accidental erasing of such historically important data.

The Tribune continues "Despite their null result in Dr. Pons's lab, Dr. Salamon and Dr. Bergeson are setting up a lab at the National Cold Fusion Institute. The scientists say they will not be operating any fusion cells themselves but they will be assisting others in looking for fusion particles." So far as I can make out the Institute will run almost as a funding agency with various groups being semi-independent. Thus Dr. Salamon will be a group which will be called on whenever another group finds a heat excess and his group will make independent measurements, usually of different fusion products. This seems an excellent procedure - and I hope my understanding is correct!

It strains one's belief that during the six weeks in May and June that Dr. Pons did not once wish to do a test which made a cell active, especially when he knew that his colleagues were making a test during these six weeks. However

if we assume it were true, then Dr. Pons has just said "we purposely kept the power amounts low on those cells". Now "keeping low" normally means that the power was not zero. What is the smallest possible power that Dr. Pons could measure? Certainly more than a one milliwatt (this surely will be given in their forthcoming paper). Now at a milliwatt of power from fusion, this would imply a flux of over a billion neutrons per second. Since this enormous signal was not observed, then from the results of Dr Salamon et al. and the statement of Dr. Pons, it must be concluded that the heat they observe cannot originate from nuclear fusion.

7 DISCUSSSION OF POSITIVE RESULTS REPORTED.

7.1 Florida results contested. Upstairs/Downstairs.

In May I heard that at the University of Florida in Gainsville, evidence had been found for tritium in cells. However I then heard that there were two groups working in the Nuclear Science building.

It was the Downstairs group of G. Schoessow and J. A. Wethington, both Emeritus professors of Nuclear Engineering, who had held a press conference to announce the observation of tritium in their one cell which had been running for a few days.

The Upstairs group consisted of 9 people, who are a mixed team of nuclear chemists, electrochemists, condensed matter physicists and nuclear physicists. They had been running much longer and studying calorimetry, neutrons. gammas and tritium. They did not find any effect. They also pointed out that the Downstairs group were working with heavy water that was heavily contaminated with tritium.

Felt that this affair was closed so commented very little about it.

However on 4 August on the KUER radio fusion update programme of NPR, Prof. Schoessow said that they claimed to see tritium. In the CNF summary part 1, version 2 is written "After 48 electrolysis, they find about 1 E9 tritons. After 100 hours, they find about 2 E10 tritons. A control run without current produced negligible tritium. They subjected the the Pd to a 'special treatment' before the experiment but are uncertain which 'adaptation may have contributed to
their findings.'"

" On the radio he said that he has a cold fusion cell of his own design, patent pending. He claims he can vary the heat output continuously, and on demand, from no excess heat to 200% of the input energy. He has measured tritium at a rate of 50 000 disintegrations per minute per ml of electrolyte. He said he has deliberately not said much about his work. He did not want it to leak out before he was sure of his results. His university had asked him not to speak to the press, but he had granted an interview to NPR before the request was made. He went on to say he had been called by many companies that are trying to get a cold fusion cell working. He said they often have 20 or more cells running and cannot get any effect.

Prof. Bockris has been reporting to me that the observations of tritium at Florida confirm their own work in Texas A&M. He feels they are the most advanced series as they have heat, tritium, neutrons and gamma rays. He says he is most impressed by them especially as they are respectively 81 and 75 years old (though still younger than Lineus Pauling who had a paper on Cold Fusion published in Nature at the age of 88 years). The results of the Downstairs group have apparently made a great impression in Utah and greatly helped the new National Cold Fusion Institute. Prof. Bockris says he is going to visit them these days.

I have sent a message to them asking for copies of their results as it is not possible to judge from the indirect messages I have received.

The Upstairs group have continued their measurements with one to three cells. They find no evidence for cold fusion and therefore do not endorse the claims of Drs. Schoessow and Wethington. In particular their methods are different.

The situation seems unclear but since they are neighbours it should be possible to arrange for an internal workshop to discuss exhaustively the

two sets of experiments. Naturally I am sure that in these circumstances it would be best for the reputation of the Department, if there were no publications, press releases or TV interviews.

PS This evening, 30 August, received a message that the Upstairs group leader, Dr. Muga and Dr Achey have talked with professors Schoessow and Wethington who said that they only claimed excess heat and they did not claim particles or tritium. When asked specifically about tritium, Prof. Schoessow replied "We do not want to see tritium".

7.2 The Texas A&M Tritium Results

Prof. Bockris sent me the preprint of their paper "Production of Tritium from D2O Electrolysis at a Palladium Cathode" by N. J. C Packham et al. which they had submitted to Nature but the referees reports were unsympathetic. He asked for my opinion. The paper reports on 24 cells tried. Results of measurements for tritium for nine of them are tabulated for a variety of running conditions and activities up to 5 E7 disintegrations per minute per ml were given. These are very high rates and my immediate concern was for the health of the experimenters. If the tritium was from the reaction

d + d ---> t + p

then the 3 MeV protons would give large numbers of neutrons from the charge exchange reaction, p + n ---> n + p , and the 1 MeV tritons would produce neutrons from t + d ---> n + 4He. A serious biohazard.

There were checks that the initial materials did not contain tritium, but the obvious check that has been many times discussed (e.g. Nature) to repeat the measurements with ordinary water, H2O, had not been done. Also the check where the cathode was replaced by an inert material such as platinium had not been done. As a fairly experienced referee and editor, this was sufficient grounds for asking for further work as when one claims an important result, normal standards of refereeing should apply. In addition the paper was not up to normal standards in several other respects.

7.3 General Electric

It was written in a confidential report that was obtained by the SL Tribune, that GE personel had found 15% excess heat. However in a radio interview, Hugo Rossi, the new Director of the National Cold Fusion Institute explained that he had written the letter himself to the Council. He said that the GE scientists had been given three P&F type cells prepared in Pons's own lab, one of which (according to P&F's measurements) consistently produced 15% excess heat. The three cells were taken back to Schenectady for GE scientists to make their own measurements. There was evidently some question in the minds of the GE people over the calibration and Rossi stated that if a conservative view was taken, the 15% would evaporate. Rossi made it clear that GE scientists had not confirmed excess heat.

7.4 Other Positive Results

7.4.1,2,3 The three positive results that most impressed people at the EPRI meeting were from Florida, Texas and GE and they have been discussed above. About the other quoted results;

7.4.4 Utah - see measurements of Salamon et al. discussed earlier.

7.4.5 Drs Storms and Talcott at Los Alamos National Lab. The Lab has called these results preliminary. It is said that after the first two cells subsequent cells (60 of them!) had failed to show an effect.

7.4.6 Dr. Scott of Oak Ridge National Lab - the Lab spokesman called these results "preliminary"

7.4.7 Dr. Mackubre of Stanford Research Institute has claimed 8% excess heat. The EPRI funding agency described them as "some indications of heat, but they have been sporadic". More information is necesary.

7.4.8 At Case Western Reserve University, "low levels of excess energy" have been reported - more information is necessary.

7.4.9 Tritium production at the University of Colorado at levels well below that corresponding to claims of excess heat - again more information is necesary to judge

7.4.10 Prof. Huggins of Stanford University has reported 12% excess heat based on observing that the cathode got warmer when D2O was used than when H2O was used - however this is natural given the different properties (e.g. heat capacities) of the two systems - but await more information, including these effects being taken into account.

7.5 Summary of Positive and Negative Results

There are a number of experiments that have given positive results, but if one asks that the experiments have all the checks done (including H2O and dummy cathodes), be well analysed with conventional statistics and show a statistical significant effect consistently, then I do not know of one which satisfies these normal scientific requirements.

On the other hand there are groups which have done careful work, explained it, obtained consistent results, but these groups all obtain null results i.e. they observe no cold fusion effects. Examples are Harwell, Karlsruhe, Yale-BNL, ATT, Bugey-Frejus, Caltech and several others.

Many of the groups obtaining positive results emphasise that the effects they observe are sporadic and irregular. They cannot predict when they happen. Normally this is taken as a sign that the results are untrustworthy. But this is turned into an advantage saying it shows this is something new and needs further investigation. But before further investigation it is necessary to establish that any effect is taking place at all and this means doing an experiment that meets with the criteria of normal science. If one wishes to believe the effect is caused by fusion then a reasonable experiment would try to observe more than one fusion product simultaneously and do the elementary check that the rates deduced from each fusion product are in agreement.

If only there were one good experiment that was well done and well described. Just one.

8. OTHER ITEMS

8.1 Visit to Harwell

Visited Harwell and spent some time with David Williams. Discussed the Harwell work - it is very impressive and extensive. They have written a major paper for Nature, but when I checked with David today, they still had not heard whether it has been accepted. But the essential results are they they do not see any excess heat, neutrons, gammas, tritium or helium and have been able to give upper limits on processes which are very low.

8.2 Report from Berlin

Prof Marx of the Freie University of Berlin (who had worked with Prof. Kreysa of Frankfurt) told me that they had put tritium also in their cell and found that they quickly had DTO. They looked for neutrons and found "nothing, absolutely nothing"

8.3 Comments on DOE Panel report by Dr Pons and Senator Garn

The Deseret News printed comments on the interim report of the Department of Energy Panel by Dr. Pons;

"'It's a totally useless committee that is telling untruths. It serves no purpose.' He said he agreed with a statement made by Senator Jake Garn, R-Utah.

Garn critised what he sees as dirty politics in the scientific community for smearing cold fusion research and ruining its chances for large federal spending.

'I used to think politicians were dirty, slimy dishonest people. I've decided that the scientists are far worse than politicians', Garn said.

'I wholeheartedly agree with him' Pons said. 'In fact I am thinking about

changing professions and becoming a politician'".
[and I used to think US Senators were distinguished responsible people]
Actually Dr. Pons had started attacking the Panel in the Salt Lake Tribune the day before, agreeing with Dr. Bockris that it was a "killer commission" and attacking Nature editor John Maddox especially because of his editorial calling on scientists 'to dismiss cold fusion as an illusion'. "'John Maddox's problem is he only reads his own newspaper", Dr. Maddox said, emphasising that Nature 'is a newspaper not a scientific journal.'"
[Pity, I've just ordered Nature and rather enjoy it].

8.4 Paper from Madrid

In June was invited to go to Madrid where the group of Carlos Sanchez had detected neutrons with a BF3 detector plus some gammas and variations in tritium concentration. They have now sent me a copy of their paper which will be published in Solid State Comm. The paper gives the data and at the foot of the table is written "After finishing this record (on 9 June) the current was switched on and the experiment is still running in the same conditions. From that day until now only background-like signals have been recorded from detectors and counters." [Carlos is a very kind man and I was most impressed by a physics course that he has set up for blind students, which particularly interested me. There are also practical demonstrations that he has built. In Note No. 15, I asked if any other university had a similar course for blind students. So far I have had no replies - does this mean that Carlos's course is the first?]

8.5 Changes at Los Alamos

In the subject of cold fusion, Los Alamos has tried to be very fair and even-handed. However at the Santa Fe conference that they mainly organised with varible success, people from Los Alamos presented results which both established cold fusion and showed that it did not occur. The null results have not, as far as I know, been questioned, but the reports of positive results have been controversial. At the meeting, after the evidence for neutron bursts had been presented, I asked if the essential control experiment of using normal H2O had been done and was told by Dr. Menlove that it had not been done but they would do it next. However they have now issued a preprint in which other experiments have been added but this promised check is not reported. Have asked their BYU collaborator several times about this but so far have not had a reply. Now I hear from various sources that they have trouble repeating these sporadic bursts - it would be good to have this cleared up. There is also considerable confusion over the press reports that Drs Storms and Talcott had found tritium but the Lab called the results preliminary. Again they may be having trouble repeating their results. The Wall Street Journal of 19 July carried an advertisement for a Director and Deputy Director of Public Affairs.
[It is often difficult to be fair and balanced. For example if you were making a film where the earth is shown to be round, would you agree to a demand from a member of theFlat Earth Society for equal space to show the earth was flat?]

8.6 European Physical Society

It is interesting to see how opinion among physicists changes with time about cold fusion. In May the American Physical Society organised two sessions on cold fusion but for their bi-annual meeting next month the European Physical Society has decided that cold fusion should not be discussed.

8.7 High Altitude Cold Fusion

In the August 21 edition of Community College Week, it is reported that at Colorado Mountain College they are trying cold fusion in their science lab which is at 10 000 feet. This is part of a large project by Rockwell International to test several variations of the Utah experiment. Associate professor Peter Jeschofnig and student volunteers have

monitored the cold fusion experiment using equipment supplied by Rockwell. The company collected the data files as well as gases generated by the experiment to conduct further research. Dr. Jeschofnig said "the college altitude test did appear to generate fusion. It looks like a positive test" he added. Rockwell received funding for its test through the Department of Energy. At other sites the company is using several other variations in addition to altitude.

The aim of the study at high altitude was to look for muon-generated fusion. It is possible that it was not known that more decisive and better controlled experiments have been performed and reported on of tests on cold fusion using muon beams. Also theoretically one would expect the muons to be quickly trapped on the high-Z palladium cathode.

Douglas R. O. Morrison

PS There is no truth in the rumour that the five-foot cathode is really intended for Kali.
PPS Some scientists in Utah have begun to refer to the inconsistencies coming out of the Cold Fusion establishment as HEAVYWATERGATE.

NOTE ON LEP

During the five or so days of the pilot run of LEP, some collisions were obtained during less than 16 hours and the four experiments found a total of over 50 examples of Z0 events. Commissioning of LEP is continuing and a beam of over one milliAmp has been obtained. It is planned to start a physics run on 11 September.

Received: from CEARN by CEARN.cern.ch (Mailer R2.03B) with BSMTP id 4611; Fri, 06 Oct 89 23:56:34 GVA
Received: from mint.cern by CEARN.cern.ch (IBM VM SMTP R1.2.1) with TCP; Fri, 1 Jun 89 23:56:29 GVA
Received: from cernvax.uucp (cernvax) by mint.cern (cernvax) (4.12/3.14)

id AA16356; Fri, 6 Oct 89 23:54:54 +0100
Received: by cernvax.uucp (5.57/Ultrix2.0-B)

id AA06714; Fri, 6 Oct 89 23:53:34 +0100
Message-Id: <8910062253.AA06714@cernvax.uucp>
Date: Fri, 6 Oct 89 23:50 GMT +1
From: MORRISON%VXPRIX.decnet.cern.ch@cernvax
Subject: LEP News.
To: RLG2@YKTVMV
X-Vms-To: MINT::"RLG2@YKTVMV"

Dear E632 and WA84 Collaborators 6 October 1989.

LEP NEWS.

On Wednesday there was the Open Session of the LEP Advisory Committee. Emilio Picasso gave a summary talk on the present LEP situation and more detailed talks were given by Steve Myers and Albert Hoffman. The machine has had initial peak luminosities of up to 8 E 29 and an average luminosity of about 2 E 29. Theoretically this would make available 1443 Z0's for each experiment at the peak energy of 45.625 GeV (but later it was said that the luminosities are not equal in all regions). Also there have been runs at other energies, +/- 1 GeV and +/-2 GeV giving five energies where about 100 to 300 Z0's could theoretically have been observed. After the shutdown from the 9 to 19 October there will be running until Christmas. Steve Myers hopes that they will reach their design luminosity of 3 mA in each beam by then so that he can win his bet with Burt Richter of 10 pounds. The SC low beta has just

started to work bring beta down from 20 cm to 7 cm. They aim to get to about 4.3 mA per beam.

John Thresher described the experimental side. In the pilot run in August in 15 hours running time the Z0 count was;

Aleph	15
Delphi	6
L3	16
Opal	21

Total 58 Z0's.

During this period the average luminosity was 1 E 28 and the lifetime of the beam was about 2 hours.

In the first period of the physics run, 20 to 25 September they had 42 1/2 hours of beam at 91.25 GeV with an average luminosity of 2 E 29.

In the second period, 28 September to 2 October there was 51 1/2 hours at 5 energies between 89.25 and 93.25 GeV with an average luminosity of 2 E29 (note that as the lifetime of the beam increases, the average luminosity decreases as the beam is run longer!). It is hoped that this will give the Z0 mass and width to 100 MeV if the energy calibration is good (they are working on this).

In the third period, 5 to 9 October, it is intended to run with the low beta at 7 cm which should increase the luminosity (am told the luminosity has in fact greatly increased).

So far in the first two periods, the approximate number of Z0's are;

Aleph	1500
Delphi	800
L3	1000
Opal	1800

giving a total of over 5000.

Various graphs were shown of thrust, sphericity etc. which agreed with most Monte Carlo predictions. Aleph had a nice (pi+pi-) mass distribution which gave a narrow peak at the K0 mass.

Groups are working on their first paper giving the Z0 mass and width and the cross section. Expect at least two papers in the next few days. Naturally there are rumours as to what the number of neutrino families will be, but we should wait on the papers. I have a bet with the Astrophysicist, Prof Peebles, so should perhaps prepare two telegrams to send to him according to the result, but it is a symbolic bet of one dollar.

Overall everyone is very happy and are praising LEP.

Douglas

==========

Date: 10 October 1989, 15:40:17 EDT
From: (R.L.Garwin (914) 945-2555) RLG2 at YKTVMV
IBM Fellow and Science Advisor to the Director of Research
P.O. Box 218
Yorktown Hts, NY 10598
To: DROMCD at CERNVM
Subject: Progress?
Reply-To: RLG2 at WATSON

I suggest seriously that you will be in an excellent situation to write a book on the Cold Fusion Caper.

I have just talked with P.K. Iyengar yesterday, who assures me that BARC still sees neutrons and tritium, but with 10**-8 as many neutrons as tritons. Confronted with the 10**-5 to 10**-4 yield of 14-MeV neutrons expected from

1.0 MeV Ts born in TiD or D2O, he invokes "channeling" or something to suppress them. But BARC is the most credible evidence.

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Dokl. Akad. Nauk SSSR Vol 307, pp99-101 (July 1989) has a paper by P.I. Golubnichii, et al, advocating cracking as a means of accelerating D for fusion. Not persuasive. But pp. 369-370 is quite exciting (M.A. Yaroslavskii) in presenting "Neutron Emission During Plastic Deformation of Deuterium-containing Solids Under Pressure." He twists the anvils of a press at liquid N2 temperature and finds bursts of thousands of counts in a neutron counter.

I'll send you the Mol paper and the translations of Doklady (by Don McNeill, Princeton).

Dick Garwin

==========

Received: from CEARN by CEARN.cern.ch (Mailer R2.03B) with BSMTP id 9257; Fri, 13 Oct 89 19:00:38 GVA
Received: from mint.cern by CEARN.cern.ch (IBM VM SMTP R1.2.1) with TCP; Fri, 13 Oct 89 19:00:20 GVA
Received: from cernvax.uucp (cernvax) by mint.cern (cernvax) (4.12/3.14)

id AA15692; Fri, 13 Oct 89 18:58:30 +0100
Received: by cernvax.uucp (5.57/Ultrix2.0-B)

id AA16886; Fri, 13 Oct 89 18:55:24 +0100
Message-Id: <8910131755.AA16886@cernvax.uucp>
Date: Fri, 13 Oct 89 18:52 GMT +1
From: MORRISON%VXPRIX.decnet.cern.ch@cernvax
Subject: LEP Results.
To: rlg2@yktvmv
X-Vms-To: MINT::"rlg2@yktvmv"

Dear E632 and WA84 Colleagues, Friday, 13 October 1989.

LEP NEWS

This afternoon the four LEP groups presented their first results. Some of the groups had already sent their papers off Wednesday and Thursday so that some of the results were known, though not widely disseminated. By a coincidence SLAC held a press conference yesterday and gave results based on some 500 events, I was told.

The numbers of Z0's decaying into hadronic modes per experiment and the number of Bhabhas, were;

	Z0	Bhahbas
Aleph	3112	
Delphi	1038	1681
L3	2538	12472
Opal	4359	

It should be noted that the luminosity was not the same in all crossing regions. It is a tremendous achievement to have these vast detectors running efficiently so soon after their first test run so it is not too surprising

that Opal which had the most robust technique, had also the largest number of events while Delphi which has the most innovative components, has the least at this stage.

The masses and widths in GeV found for the Z0 (plus earlier results from CDF) were;

	Mass		Width
Mark 2	91.14 +/- 0.12		2.42 +0.45/-0.35
CDF	90.9 +/- 0.3	+/- 0.2	3.8 +/- 0.8 +/- 1.0
Aleph	91.170 +/- 0.054		2.68 +/- 0.15
Delphi	91.09 +/- 0.10		2.29 +/- 0.24
L3	91.132 +/- 0.057		2.588 +/- 0.137
Opal	91.010 +/- 0.051		2.60 +/- 0.13

Combining the four LEP results, John Thresher found
91.10 +/- 0.06

To these values of the Z0 mass should be added a systematic uncertainty from LEP of 0.046 GeV. The above are three-parameter fits.

The values for number of neutrino families found were;

Mark 2	2.8 +/- 0.6
Aleph	3.27 +/- 0.30
Delphi	2.91 +/- 0.70
L3	3.42 +/- 0.48
Opal	3.12 +/- 0.42

Conbining the four LEP results, John Thresher found
3.25 +/- 0.22
This clearly excludes four neutrino families. Alvaro asked when the error would be such that one could tell if 3.25 was significantly different from 3. He was told that it is hoped to have 100 000 Z0 events by the end of the year and perhaps a million by the end of next year! (a luminosity of 1 E30 has already been reached).

In the estimate of the number of neutrino families, there are some assumptions involved which may help to explain why there are some apparent discrepancies between the errors and the number of Z0 events.

The partial widths in MeV, into Gamma(mu-mu) and Gamma(ee) were measured by L3 to be 92 +/-6 and 88 +/- 9 +/- 7 resp.

Many other results were presented. In particular distributions of Thrust, Sphericity, etc were found to be in good agreement with the Standard Model.

Some of these results were taken from papers, some were copied from the transpariencies shown, hence they should all be treated with caution.

Best Wishes,
Douglas.

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Received: from CEARN by CEARN.cern.ch (Mailer R2.03B) with BSMTP id 8902; Fri, 20 Oct 89 20:34:46 GVA
Received: from mint.cern by CEARN.cern.ch (IBM VM SMTP R1.2.1) with TCP; Fri, 20 Oct 89 20:34:44 GVA
Received: from cernvax.uucp (cernvax) by mint.cern (cernvax) (4.12/3.14)

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id AA21674; Fri, 20 Oct 89 20:35:50 +0100
 Received: by cernvax.uucp (5.57/Ultrix2.0-B)

id AA11466; Fri, 20 Oct 89 20:35:58 +0100
 Message-Id: <8910201935.AA11466@cernvax.uucp>
 Date: Fri, 20 Oct 89 20:32 GMT +1
 From: MORRISON%VXPRIX.decnet.cern.ch@cernvax
 Subject: Cold Fusion.
 To: rlg2@yktvmv
 X-Vms-To: MINT::"rlg2@yktvmv"


 Dear Dick,                                                  20 October 1989.
          Thanks for your message and the interesting information.
 Have just started this evening to write no.20 of my notes - have been very busy
 with scintillating fibres plus giving talks on Cold Fusion at Utah, Texas A&M
 and Gainsville.
   Hope to see you when you come to CERN next week for George Charpak's Fest.
      Best Wishes,
             Douglas.

 PS was lucky your message reached me as my address is on Vax and not VM
 but they kindly forwarded it by hand.
   the address is;
                    DROMCD@VXCERN.DECNET.CERN.CH

 This one is automatically forwarded to my favourite quiet address;

                 MORRISON@VXPRIX.DECNET.CERN.CH

================================================================================
Date: 21 October 1989, 18:34:15 EDT
From: (R.L.Garwin               (914) 945-2555)      RLG2     at YKTVMV
      IBM Fellow and Science Advisor to the Director of Research
      P.O. Box 218
      Yorktown Hts, NY  10598
To:   dromcd   at vxcern.decnet.cern.ch
Subject: Trial of your new address.
Reply-To: rlg2@ibm.com

I have always reached you via DROMCD AT CERNVM.  But I will try this address.
Please let me know if this reaches you.

Dick Garwin
==============
--------------------------------------------------------------------------------
 Received: from CEARN by CEARN.cern.ch (Mailer R2.03B) with BSMTP id 4429; Sun,
  22 Oct 89 22:08:30 GVA
 Received: from mint.cern by CEARN.cern.ch (IBM VM SMTP R1.2.1) with TCP; Sun, 22
  Oct 89 22:08:28 GVA
 Received: from cernvax.uucp (cernvax) by mint.cern (cernvax) (4.12/3.14)

id AA03238; Sun, 22 Oct 89 22:09:32 +0100
 Received: by cernvax.uucp (5.57/Ultrix2.0-B)

id AA20890; Sun, 22 Oct 89 22:09:09 +0100
 Message-Id: <8910222109.AA20890@cernvax.uucp>
 Date: Sun, 22 Oct 89 22:05 GMT +1
 From: MORRISON%VXPRIX.decnet.cern.ch@cernvax
 Subject: Message from CERN Postmaster.
 To: rlg2@yktvmv
```

X-Vms-To: MINT::"rlg2@yktvmv"

Dear Dick, 22 October 1989.
This is the message that I received - guess they just forwarded the others without fuss previously,
Regards,
Douglas.

From:
VXGIFT::MINT::"POSTMAST@CERNVM.cern.ch" 20-OCT-1989 16:34:28.34
To:
morrison@VXPRIX.decnet.cern.ch
CC:
Subj:

Date: Fri, 20 Oct 89 16:37:40 +0100
From: POSTMAST@CERNVM.cern.ch
Resent-date: Fri, 20 Oct 89 16:36:55 SET
Resent-from: POSTMAST@CERNVM.cern.ch
Resent-to: morrison@VXPRIX.decnet.cern.ch
Resent-Message-Id: <8910201537.AA19234@mint.cern>
Message-Id: <8910201537.AA19234@mint.cern>
Passed-Date: Fri, 20 Oct 89 16:30:52 SET
Passed-From: POSTMAST@CERNVM
Passed-To: MORRISON@CERNVM
Apparently-To: <@MINT:morrison@VXPRIX.DECNET.CERN.CH>

Mail has had to be delivered to you manually for the following reason:

```
**********************************************************************
* User:   DROMCD@CERNVM   does not exist                             *
*                                                                    *
* Please update your entry in the electronic mail directory EMDIR *
* so that remote users and the Postmaster can find your preferred *
* mail address.                                                      *
*                                                                    *
**********************************************************************
```

Please make sure that your correspondent is informed of your correct mail address.

Please note that this forwarding service will shortly be discontinued.

Regards,
Postmaster
---------- FORWARDED MAIL FILE ----------

Date: 10 October 1989, 15:40:17 EDT
From: (R.L.Garwin (914) 945-2555) RLG2 at YKTVMV
IBM Fellow and Science Advisor to the Director of Research
P.O. Box 218
Yorktown Hts, NY 10598
To: DROMCD at CERNVM
Subject: Progress?
Reply-To: RLG2 at WATSON

----------Original message----------
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Dick Garwin
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Received: from mcsun.EU.net by IBM.COM (IBM VM SMTP R1.2.1MX) with TCP; Sun, 22 Oct 89 14:46:01 PDT
Received: by mcsun.EU.net via EUnet; Sun, 22 Oct 89 22:46:26 +0100 (MET)
From: MORRISON@VXPRIX.decnet.cern.ch
Received: by cernvax.uucp (5.57/Ultrix2.0-B)

id AA12604; Sun, 22 Oct 89 21:56:42 +0100
Message-Id: <8910222056.AA12604@cernvax.uucp>
Date: Sun, 22 Oct 89 21:53 GMT +1
Subject: RE: Trial of your new address.
To: rlg2@ibm.com
X-Vms-To: VXCERN::VXGIFT::MINT::"rlg2@ibm.com"

Yes, your message reached me via dromcd@vxcern Willtry and mail you the letter I received. Best Wishes,
Douglas.

Energy Research Advisory Board
to the
United States Department of Energy
1000 Independence Avenue, S.W.
Washington, D.C. 20585
(202) 586-5444

August 21, 1989

TO: Members of the Cold Fusion Panel

The enclosed is circulated for your information:

1. Letter from Dr. Garwin to Drs. Fleischmann and Pons.
2. Submission from J. Rand McNally.
3. Papers from Peter Hagelstein.

Tom Finn for

William L. Woodard
Panel Secretary

Enclosures

Richard L. Garwin
IBM Research Division
Thomas J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY 10598
(914) 945-2555

August 16, 1989
(Via FAX to 9 (801) 581-8433)

Professor Martin Fleischmann
Professor Stanley Pons
Department of Chemistry
2416 Henry Eyring Bldg.
University of Utah
Salt Lake City, UT 84112

Dear Professor Fleischmann or Professor Pons:

As a member of the Subcommittee of the ERAB Cold Fusion Panel charged with evaluating work done on particle emission in cold fusion experiments, I need some specific information that only you can provide. Referring to your original publication in Journal of Electroanalytical Chemistry, I see the gamma ray peak at 2.2 MeV. This has, of course, been reprinted in Nature by Petrasso and co-workers.

But in your Nature article of 06/29/89, you show your signal peak at "2.496 MeV."

In our *understanding of* results from laboratories all over the world, it is important to us to be aware of problems and sources of error that might affect these results. Therefore I would be most grateful to receive by September 1 a full explanation of this discrepancy.

Thank you very much.

Sincerely yours,

Richard L. Garwin
Forwarded in his absence

cc:
W.L. Woodard, DOE (Via FAX to 9 (202) 586-3119)

RLG:jah:228%MF:081689..MF

Also Adjunct Professor of Physics at Columbia University
(Views not necessarily those of IBM or Columbia)

+ to be presented at Knoxville IEEE meeting, Oct 3, 1989

COLD FUSION PROSPECTS

J. Rand McNally, Jr.
Fusion Energy Consultant
103 Norman Lane
Oak Ridge, TN 37830

Abstract

Cold fusion prospects may be based in part on a nuclear mass-energy resonance with an excitation level in the compound nucleus or in two output nuclei; very high density; a large neutron capture cross-section isotope and a neutron-rich isotope; overlapping de Broglie interaction wavelengths ($\lambda = h/Mv$); nuclear spin and polarizability compatibility; and/or catalytic action by a passing electron, neutron, or gamma ray [1, 2]. The most notable nuclear mass-energy resonance is D + T = $^5He^*$(16.70 MeV) which occurs 60 keV below the 16.76 MeV excitation state in $^5He^*$, and has a maximum cross-section of 5 barns at 60 keV center-of-mass energy of D + T. However, the level width is 100 keV ± 50 keV so that perhaps some cold fusion of D + T could be produced at very low energies in dense media. High density enhances the reaction rate ($R = n_D n_T \sigma v$ or $n^2 \sigma v/2$). It has been estimated that pycnonuclear fusion of D + T and D + D can ignite at densities of 10^4 and 10^5 g/cm^3, respectively [3]. Other prospects, including the cold fusion of D + D, will be discussed.

Text

The first paper to outline some of the requirements for attaining cold fusion was published in 1985 [1]. After the publication of several experimental claims of obtaining cold fusion results [4], a second paper was published which described possible mechanisms for producing D + D fusion at room temperature and high densities ($n \sim 10^{22}$ D/cm^3) [2]. At very high densities (10^4 or 10^5 g/cm^3 for D + T or D + D fusion ignition) and room temperature, pycnonuclear fusion may occur [3].

The requirements for cold fusion to occur may include, but are not limited to: 1. a nuclear mass-energy resonance between the mass-energies of the input nuclei (Mc^2) and the mass-energy of the compound nucleus with the excess energy appearing as excitation energy of the compound nucleus; 2. high density to offset the low cross-section to be expected; 3. a large neutron capture cross-section isotope with a neutron-rich isotope; 4. overlapping de Broglie interaction wavelengths ($\lambda = h/Mv$); 5. nuclear spin and polarizability compatibility; and/or 6. catalytic action by a passing electron, neutron or gamma ray.

Nuclear mass-energy resonances may be obtained by consulting the various publications on energy levels in Nuclear Physics for possible resonances with the input nuclei and the output compound nucleus (or nuclei) having an excitation level(s) in which the net excess of energy is within the level width of the excited state(s). Thus, for D + T → $^5He^*$ one has

$$M_D c^2 + M_T c^2 - M_{^5He} c^2 = \Delta E_{\text{exc. of } ^5He^*}$$

The $^5He^*$ level has an excitation energy level at 16.76 ± 0.13 MeV [5] compared to ΔE = 16.70 MeV. Thus, the 5 barn cross-section at 0.06 MeV center-of-mass energy of D + T is directly attributable to this mass-energy resonance. The level width of the 16.76 MeV level is 0.1 ± 0.05 MeV so that an overlap of the level width with room temperature D + T nuclei may permit a nuclear mass-energy resonance and cold fusion to be possible at high densities. The 16.76 MeV excitation level decays most of the time by n + α emission with the release of 17.65 MeV of kinetic energy; but about 0.021% of the time it decays by emitting a 16.76 MeV gamma ray for deuteron bombarding energy in the range 25-100 keV.

The nuclear mass-energy defects ($\Delta M = \Delta M' c^2$ in keV) may be obtained by consulting the tables of Wapstra and Gove [6]. The relation is then

$$\Delta M_D + \Delta M_T - \Delta M_{^5He} = \Delta E_{\text{exc. of } ^5He^*}.$$

The case of the cold fusion of D + D, which has merited a great deal of attention in various news and television releases [see 4], is somewhat similar.

On the Possibility of Coupling Nuclear Fusion Energy to Phonons

Peter L. Hagelstein
Research Laboratory of Electronics
Massachusetts Institute of Technology
Cambridge, Massachusetts 02139

Manuscript revision date: August 8, 1989

ABSTRACT

The coupling of nuclear energy from fusion reactions to phonon modes in a lattice is examined. If the interaction matrix element for (off-resonant) single phonon emission has a magnitude larger than the phonon energy, then the possibility exists that phonons may be generated coherently at an accelerated rate.

I. Introduction

Fleischmann and Pons have recently claimed to have demonstrated heat production through nuclear fusion at near room temperature in deuterium-filled palladium rods.[1] There have been a number of reports of the observation of "cold fusion" effects in similar electrolysis experiments,[2–7] and in gas cells.[8–11] There have also many a very large number of experiments which have attempted to reproduce the effects with no success whatsoever.[12–15]

At this point it is not generally accepted either that there is cold fusion or that cold fusion effects are real. There is currently no understanding of what could possibly be responsible fusion at room temperature in an electrolysis cell. The fracture fusion mechanism[16–20] has been offered to account for some of the observations, but it is hard to understand how fracture fusion could operate in a metal to produce sufficient quantities of deuterons at high enough energy to account for the reported levels of neutron or tritium (much less heat) production.

In this paper we discuss the possibility that fusion energy can be coupled coherently into phonon modes. We recognized shortly after the initial announcement of the Pons and Fleischmann experiment that there was at least hypothetically a possibility that fusion could proceed coherently.[21] The principal drawback to such a scenario was that it seemed necessary that a very large number of phonons had to be emitted at the same time that a single fusion occured, and there seemed to be no mechanism by which this could occur.

In this paper we examine a scheme in which a very large number of phonons are generated coherently, one phonon at a time. We find that this process can occur as long as there is an interaction matrix element for the off-resonance emission or absorption of a phonon for each fusion or unfusion transition which is larger in magnitude than the phonon energy.

II. Quantum Mechanics for Nearly Degenerate Levels

Let us consider a simple model of a lattice which contains a large number of nucleon pairs which have at least one exothermal fusion channel available, and whose fusion products have the possibility of remaining localized relative to one another in the event that the nuclear energy produced could be taken up by the lattice. For example, the fusion of d+d to produce p+t satisfies the conditions proposed, while the fusion of d+d to produce ^{3}He + n does not since the neutron cannot remain localized.

A macroscopic lattice possesses a very large number of phonon modes, but in our simplified model we shall focus on just one mode. The extension of this type of model to include multiple phonon modes is straightforward and adds very little to the essential physics.

We shall use a superposition of states to describe the coupled lattice/nuclear system:

$$\Psi(t) = \sum_n \sum_m c_{nm}(t) \Phi_{nm} e^{-\frac{iE_{nm}t}{\hbar}} \tag{1}$$

Here we have adopted the quantum number n to describe the number of fusions which have occured in the lattice, and m to describe the number of phonons in the mode of interest. In this toy model, the eigenfunctions Φ_{nm} satisfy

$$H_o \Phi_{nm} = E_{nm} \Phi_{nm} \tag{2}$$

where H_o is the many-particle hamiltonian stripped of terms connecting states of different fusion number. For simplicity, one might picture these eigenfunctions as having no phonons in other modes. The additional machinary required to include a more realistic lattice description at this point does not alter the basic physics, and the extension formally of this model to a more realistic situation is straightforward.

We now introduce interaction matrix elements connecting the states Φ_{nm} and $\Phi_{n'm'}$. For example, if d+d were to produce p+t with the excess energy being taken up by the lattice, the lattice would be perturbed by the substitution due to the differences in interaction of the lattice with a $d+d$ pair versus a $p+t$ pair. Mathematically, this appears in the form of

non-zero matrix elements between lattice states of different fusion number n and phonon number m.

The evolution equations for the amplitudes $c_{nm}(t)$ will in general include terms connecting a given state Φ_{nm} to many different states $\Phi_{n'm'}$. For example, if all of the nuclear energy were somehow to be given up at once, then a very large number of phonons would have to be generated. The matrix element associated with this process is expected to be vanishingly small. The largest interaction matrix elements will occur between states differing by the fewest number of fusions and phonons. As a result, if we include in our analysis only interactions where $n', m' = n \pm 1, m \pm 1$, we then obtain the evolution equations

$$i\hbar \frac{d}{dt} c_{n,m} =$$

$$\langle n,m \mid H \mid n+1,m+1 \rangle e^{i(-\omega_n - \omega_p)t} c_{n+1,m+1} + \langle n,m \mid H \mid n+1,m-1 \rangle e^{i(-\omega_n + \omega_p)t} c_{n+1,m-1}$$

$$+ \langle n,m \mid H \mid n-1,m+1 \rangle e^{i(\omega_n - \omega_p)t} c_{n-1,m+1} + \langle n,m \mid H \mid n-1,m-1 \rangle e^{i(\omega_n + \omega_p)t} c_{n-1,m-1} \quad (3)$$

in which ω_n and ω_p are the nuclear and phonon energies divided by $\hbar$. This equation describes virtual processes in which a fusion event is coupled to the emission or destruction of one phonon. Since the phonon energies (less than 1 eV) are small in comparison with nuclear fusion energies (more than 1 MeV), any single such transition is highly nonresonant. A very large number of phonons must be emitted for a single fusion to have occured, and in the scenario which we are developing, these phonons are emitted virtually, one at a time.

Although moderately simple, these equations are not transparent, and there is no reason *a priori* to believe that they lead to anything but a very complicated time-dependent evolution which does not particularly favor the occurance of fusion transitions. We can however make some progress if we use (2) to compute the second derivative of c_{nm}. Altogether, 32 terms arise from simply iterating (2), all of which have associated oscillatory phase factors. If we assume that the dominant behavior of the amplitudes can be found through retaining only the most slowly varying terms, we then obtain the approximate equation

$$\frac{d^2}{dt^2}c_{n,m} = -2[\frac{H}{\hbar}]^2 \left(e^{-2i\omega_p t}c_{n,m+2} + 2c_{n,m} + e^{2i\omega_p t}c_{n,m-2}\right) \tag{4}$$

where we have assumed for simplicity that the matrix elements are constant (in n and in m, locally) and real. The solution of these equations normally does not in general yield any particular acceleration of a fusion rate. But in the limit that the magnitude of the interaction matrix element is much larger than the phonon energy

$$| H | >> 2\hbar\omega_p \tag{5}$$

then the states are approximately degenerate relative to the coupling (that is the phase factors can be set to unity). The phonon generation rate is found to be

$$\gamma_p = \frac{4\sqrt{2}}{\pi}\frac{| H |}{\hbar} \tag{6}$$

Since the coupled equations (2) conserve energy, the fusion rate can be inferred from the rate at which phonons are generated; the result is

$$\gamma_f = \frac{4\sqrt{2}}{\pi}\frac{\omega_p}{\omega_n}\frac{| H |}{\hbar} \tag{7}$$

The requirement of (4) may be viewed as a fundamental constraint for the onset of coherent fusion. It would be a sufficient condition in the absence of phonon damping, which involves interactions of the lattice with external matter not included in this simple picture.

As a practical matter, the constraint of equation (4) is a relatively weak one. For example, consider a bar having a lowest acoustical mode of frequency 10^3 Hz, and let the interaction matrix element be 10^6 Hz $\times$ h. The resulting phonon generation rate is a substantial 1.1×10^7 sec^{-1}, corresponding to a fusion rate of 8×10^{-9} sec^{-1}. These numbers demonstrate that only a very small interaction matrix element is required to achieve the condition of coherency. Much larger coupling would be required to account for the cold fusion effects which have been reported. Nevertheless, from the arguments presented here, it seems possible that coherent fusion at relatively low rates may be a relatively common occurence in highly deuterated systems.

III. The Interaction Matrix Element

In the case of $E0$ coupling, the interaction matrix element between two lattice eigenfunctions is

$$\langle \Phi \mid H_I \mid \Phi_f \rangle = \langle \Phi_i \mid \sum_{jkl} \frac{Z_i e^2}{\mid \mathbf{r}_i - \mathbf{r}_{kl}^{cm} \mid} + \frac{Z_j e^2}{\mid \mathbf{r}_k - \mathbf{r}_{kl}^{cm} \mid} \mid \Phi_f \rangle \tag{8}$$

where we have assumed that the wavelength of the exchanged photon exceeds the rod dimensions as is appropriate for coupling to acoustical phonon modes. The summation index j is over all particles in the rod ($Z_j e$ is the particle charge) and the indices k and l are over fusing nucleons ($\mathbf{r}_{kl}^{cm}$ is the center of mass coordinate). If a "Hartree" splitting is used, such that Φ_i and Φ_f are taken to be product states of a two nucleon wavefunctions and a lattice wavefunction, for example

$$\Phi_i = \phi_i(\mathbf{r}_k \mathbf{r}_l) \Phi_i^{kl}(\mathbf{r}_i \cdots \mathbf{r}_j, \mathbf{r}_m \cdots \mathbf{r}_N) \tag{9}$$

then the matrix element becomes

$$\langle \Phi_i \mid H_i \mid \Phi_f \rangle \approx \sum_{jkl} \langle \phi_i \mid \phi_j \rangle \langle \Phi_i^{kl} \mid \frac{2 Z_j e^2}{\mid \langle \mathbf{r}_i \rangle - \mathbf{r}_{kl}^{cm} \mid} \mid \Phi_f^{kl} \rangle \tag{10}$$

In this result we assume that the interaction matrix element is dominated by long range contributions.

The matrix element is the product of an overlap integral and a lattice piece which is sensitive to the presence of excess charge. The overlap integral takes on values of 3×10^{-36} for an HD molecule and 2×10^{-43} for a D_2 molecule. If a neutron emission rate on the order of 10^{-24} sec^{-1} occurs, then this corresponds to an overlap of about 10^{-23}.

In order to get a very crude estimate of the magnitude of the matrix element for the $p + d \longrightarrow {}^3He + \Delta E$ (lattice) reaction, one might crudely use

$$\langle \Phi_i \mid H_I \mid \Phi_j \rangle \sim \langle \phi_i \mid \phi_j \rangle N_{pd} N_e \frac{e^2}{\bar{r}} \frac{\Delta Q}{\bar{r}} \tag{11}$$

where N_{pd} is the number of proton-deutron pairs, N_e is the number of excess charges, $\bar{r}$ is a characteritic distance to the excess charges and ΔQ is a phonon displacement amplitude. If we use $\langle \phi_i \mid \phi_j \rangle = 10^{-36}$, $N_{pd} = 10^{23}, N_e = 10^{15}, \bar{r} = 1\ mm$ and $\Delta Q = 1\mu$ (acoustical phonons), then we get an interaction energy of about 10^{-7} eV. In order to get power

generation at the watt level, most of the available phonon modes in the 100-1000 MHz range need to be involved in the coherent fusion process. Since the system has gain, it only needs to start at a low level and build up.

IV. Summary and Discussion

We have suggested that if coupling occurs between a nuclear reaction and a lattice phonon mode, then there exists the possibility that the nuclear energy can be coupled coherently, and potentially quite rapidly into the lattice. The energy exchange involves the emission of a very large number of phonons, one phonon at a time, and may occur whenever the interaction energy greatly exceeds the phonon energy in an undamped system.

From work which will be reported elsewhere, it seems that there are two basic mechanisms which may be involved in phonon emission. The possibility that d+d $\rightarrow$ p+t + ΔE(lattice) has been remarked upon in section II. Additionally, the coupled (fusion/lattice) reactions p+d $\rightarrow$ ^{3}He + ΔE(lattice) and d+d $\rightarrow$ ^{4}He + ΔE(lattice) have the potential of occuring through electromagnetic E0 coupling. We have presented here only a crude estimate of the interaction matrix element in the *E0* case here.

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A Simple Model for Coherent Fusion in the Presence of a Lattice

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ABSTRACT

A simple model for coherent fusion in the presence of a lattice is proposed. It is suggested that the reactions p+d $\rightarrow{}^3$He and d+d $\rightarrow{}^4$He can proceed through electric monopole (E0) interaction with the lattice through the emission of a very large number of acoustical phonons. This process would occur coherently, one phonon at a time. It is suggested in addition that the reaction d+d $\rightarrow$ p+t can also proceed coherently, mediated by strong force interactions and coupling to the phonons mechanically by virtue of the disparate mean interactions of the lattice with two deuterons versus a proton and triton.

I. Introduction

Fleischmann and Pons have recently claimed to have demonstrated heat production through nuclear fusion at near room temperature in deuterium-filled palladium rods.[1] There have been a number of reports of the observation of "cold fusion" effects in electrolysis experiments,[2–7] and in gas cells.[8–11] There have also many a very large number of experiments which have attempted to reproduce the effects with no success whatsoever.[12–16]

At this point it is not generally accepted that there is cold fusion or that cold fusion effects are real. There is currently no understanding of what could possibly be responsible fusion at room temperature in an electrolysis cell. The fracture fusion mechanism[17] has been offered to account for some of the observations,[18–21] but it is hard to understand how fracture fusion could operate in a metal to produce sufficient quantities of deuterons at high enough energy to account for the reported levels of neutron or tritium (much less heat) production. There have been numerous works which have been devoted towards an examination of possible explanations of cold fusion.[22–43]

Cold fusion was unexpected and if substantiated, appears to be a very dominant factor at low temperature. There is currently some interest in developing a theory to account for whatever new physics is occuring (assuming that there is new physics involved); this work is motivated towards trying to find new mechanisms by which conventional fusion can be enhanced.

The purpose of this paper is to provide a simple model for exploring coherent nuclear reactions at low temperature in the presence of a lattice. We consider in this model the fusion process to be a coherent transition between nearly degenerate macroscopic states. As a result we find that the transition rate may be proportional to the transition matrix element rather than to its square, and this results in the possibility of a considerable enhancement of the fusion rate at low temperature.

The focus of our efforts has been the exotic and new reactions p+d $\rightarrow$^{3}He + 5.5 MeV (lattice) (the primary conventional binary channel involves the emission of a gamma) and d+d $\rightarrow$^{4}He + 23.8 MeV (lattice) (binary d+d fusion reactions result primarily in either p+t or ^{3}He + n[44]). The production of ^{4}He occurs only weakly in conventional *dd*

fusion reactions following gamma emission.[45] The problem of lattice coupling in nuclear reactions has not been of particular importance to date, and we are aware of no theoretical works describing how either ^{3}He or ^{4}He might be produced following fusion without free particle (neutron, proton, triton, gamma or other particle) emission.

It is proposed that a closely related mechanism can apply to the proposed exotic reaction d+d $\rightarrow$ p + t + 4.0 MeV (lattice). There are many reactions which can proceed coherently in principle; as practical matter, the increased coulomb barriers associated with higher Z reactions probably prohibit the realization of the effect in all cases except with hydrogen isotopes.

This work is organized as follows: in Section II we discuss coherent transitions between nearly degenerate states, and present the possibility that these degenerate states may be combined nuclear/lattice states. In section III, we argue that if the fusion process is coupled to the lattice through single phonon emission at a time, then the phonon emission can be drastically accelerated if the interaction energy is much greater than the phonon energy. In sections IV-VII, we consider the development of a formulation to attack the computation of the interaction matrix element.

II. Fusion as a Coherent Process

Nuclear reaction cross-sections are normally computed using time-dependent perturbation theory for transitions to a continuum. The basic formula which results from this type of analysis is Fermi's Golden Rule

$$\gamma = \frac{2\pi}{\hbar} \mid \langle \Psi_f \mid H \mid \Psi_i \rangle \mid^2 \rho(E_f) \qquad (II.1)$$

for appropriate choices of initial and final states Ψ_i and Ψ_f. The transition rates resulting from this picture vary as the square of the interaction matrix element as is usually found in quantum mechanical calculations. Conventional binary fusion reactions follow from this type of analysis; for binary dd fusion one normally obtains a transition rate which is proportional to e^{-2G} (where G is the Gamow factor), which leads to fusion rates which are known to be extremely small within a room temperature electrolysis cell.

Coherent processes can behave very differently. For example, consider a superposition of degenerate states

$$\Psi(t) = \sum_n c_n(t) \Phi_n e^{-\frac{iEt}{\hbar}} \qquad (II.2)$$

and a static hamiltonian. The evolution equations for the amplitudes $c_n(t)$ might obey coupled equations of the form

$$i\hbar \frac{\partial}{\partial t} c_0 = \langle 0 \mid H \mid 1 \rangle c_1$$

$$i\hbar \frac{\partial}{\partial t} c_1 = \langle 1 \mid H \mid 2 \rangle c_2 + \langle 1 \mid H \mid 0 \rangle c_0$$

$$i\hbar \frac{\partial}{\partial t} c_n = \langle n \mid H \mid n+1 \rangle c_{n+1} + \langle n \mid H \mid n-1 \rangle c_{n-1} \qquad (II.3)$$

If all of the matrix elements $\langle i \mid H \mid j \rangle$ are equal, then it is simple to show that the quantity

$$\langle n \rangle \approx \sum_{n=0}^{\infty} n \mid c_n \mid^2 \qquad (II.4)$$

obeys

$$\frac{\partial^2 \langle n \rangle}{\partial t^2} = 0 \qquad (II.5)$$

for large values $\langle n \rangle$. If so, then for large $\langle n \rangle$, the first derivative must be a constant

$$\frac{\partial}{\partial t}\langle n \rangle \approx \gamma \qquad (II.6)$$

The precise value of the constant is determined from the initial conditions assuming that $c_0 = 1$ at $t = 0$. The γ can be estimated by considering $\frac{\partial \langle n \rangle}{\partial t}$ for the first two modes, and one obtains

$$\gamma \approx \frac{4}{\pi} \frac{| \langle i \mid H \mid j \rangle |}{\hbar} \qquad (II.7)$$

where the factor $4/\pi$ comes from an analytic solution of the coupled equations. In this case the transition rate is linear in the matrix element rather than quadratic.

This exercise takes on significance for the fusion problem if one considers the Φ_n states to be many-particle states of a palladium lattice filled with deuterium, where n denotes the number of fusion reactions which have occured. If a nuclear lattice reaction p+d $\rightarrow$ ^{3}He + 5.5 MeV (lattice) occurs in a palladium ball floating in space, nuclear energy is transferred to lattice vibrations conservatively. As a result no net change of macroscopic energy occurs and the two states are degenerate. Similar arguments apply if additional fusion reactions occur, and the set of macroscopic lattice states Φ_n where n fusion reactions of the types mentioned above have occured form a degenerate set.

Once the reactions get going, in the limit that the average number of reactions $\langle n \rangle$ becomes large, the reactions proceed at a rate γ given by equation (II.7). This is especially important for cold fusion where the matrix element $\langle i \mid H \mid j \rangle$ contains a small tunneling factor, and presents the possibility that very large acceleration of the fusion process might be possible.

A consequence of these considerations is that for any nuclear reaction to be coherent, no free particle emission can occur. Associated with the emission of a free particle comes a large number of final states, and once these final states dephase, the fusion process becomes

nonreversible. Implicit in the "toy model" discussed here is the notion that coupling must proceed into a single final state, and that since matrix elements for reverse processes are equal in magnitude, that the process be reversible.

One further point to note is that in order for a coherent process to "appear" stronger than a competing incoherent process, it is not necessary that the interaction matrix element be larger (as will be discussed elsewhere).

III. Emission of Phonons

Our picture of the coherent fusion process is a phonon laser scenario. The fusion energy is coupled into lattice phonons, one at a time in a rather interesting way. Let us consider a lattice in which a large number of s-wave HD molecules interact with a single phonon mode. In this case we may describe the lattice wavefunction as a superposition over both fusion (n) and phonon (m) states

$$\Psi(t) = \sum_n \sum_m c_{nm}(t)\Phi_{nm}e^{-\frac{iE_{nm}t}{\hbar}} \qquad (III.1)$$

In this case, the summation is not over degenerate states, and this is reflected in phase factors appearing in the amplitude evolution equations. We shall assume that the fusion process is dominated by virtual transitions in which one fusion and one phonon transitions dominate over all higher order processes. In this case we find that

$$i\hbar\frac{d}{dt}c_{n,m} =$$

$$\langle n,m \mid H \mid n+1,m+1\rangle e^{i(-\omega_n-\omega_p)t}c_{n+1,m+1} + \langle n,m \mid H \mid n+1,m-1\rangle e^{i(-\omega_n+\omega_p)t}c_{n+1,m-1}$$

$$+\langle n,m \mid H \mid n-1,m+1\rangle e^{i(\omega_n-\omega_p)t}c_{n-1,m+1} + \langle n,m \mid H \mid n-1,m-1\rangle e^{i(\omega_n+\omega_p)t}c_{n-1,m-1} \qquad (III.2)$$

where ω_n and ω_p are the nuclear and phonon energies divided by $\hbar$. In general, the phase factors appearing in (III.2) give rise to a transition rate with which is in general not linear in the interaction matrix element.

We can make some progress if we use (III.2) to compute the second derivative of c_{nm}. Altogether 32 terms arise from simply iterating (III.2), and all terms have associated oscillatory phase factors. If we assume that the dominant behavior of the amplitudes can be found through retaining only the most slowly varying terms, then we obtain the approximate equation

$$\frac{d^2}{dt^2}c_{n,m} = -2[\frac{H}{\hbar}]^2\left(e^{-2i\omega_p t}c_{n,m+2} + 2c_{n,m} + e^{2i\omega_p t}c_{n,m-2}\right) \qquad (III.3)$$

where we have assumed that the matrix element is locally constant and real. The solution of these equations normally do not provide any particular acceleration of a fusion rate. But in the limit that the magnitude of the interaction matrix element is much larger than the phonon energy

$$| H | >> 2\hbar\omega_p \qquad (III.4)$$

then the states are approximately degenerate relative to the coupling, and the phonon generation rate becomes

$$\gamma_p = \frac{4\sqrt{2}}{\pi}\frac{| H |}{\hbar} \qquad (III.5)$$

The associated fusion rate is

$$\gamma = \frac{4\sqrt{2}}{\pi}\frac{\omega_p}{\omega_n}\frac{| H |}{\hbar} \qquad (III.6)$$

The requirement of (III.4) may be viewed as a fundamental constraint for the onset of coherent fusion. It would be a sufficient condition in the absence of phonon damping, which involves interactions of the lattice with exterior matter not included in this simple picture.

As a practical matter, the constraint of equation (III.4) is a relatively weak one. For example, consider a bar with a lowest acoustical mode with amplitude of 10^3 Hz, and let the interaction matrix element be 10^6 Hz $\times$ h. The resulting phonon generation rate is a substantial 1.1×10^7 sec^{-1}. This corresponds to a fusion rate of 8×10^{-9} sec^{-1}. Hence coherent fusion can proceed slowly, and may be endemic in highly deuterated systems.

In order to obtain fusion rates on the order of those reported in the experiments exhibiting positive cold fusion effects, one would require much larger interaction matrix elements, higher energy phonon modes and coherent emission in a large number of phonon modes.

From these considerations, we can obtain a rough estimate of phonon frequencies which would be involved if this type of mechanism were involved in heat production as has been

claimed to have been observed. If we assume that many modes are fusing coherently (the rates for individual modes add), then the total fusion rate is

$$\gamma = \sum_j \frac{4\sqrt{2}}{\pi} \frac{\omega_j}{\omega_n} \frac{H_j}{\hbar} \qquad (III.7)$$

If we assume that the interaction matrix element is a large multiple of the phonon energy ($H_j = \eta_j \hbar \omega_j$), then the total fusion rate is

$$\gamma = \sum_j \frac{4\sqrt{2}}{\pi} \eta_j \frac{\omega_j^2}{\omega_n} \qquad (III.8)$$

The number of modes which are available at a phonon frequency ω_j is on the order of $3V\omega_j^2 d\omega_j/(2\pi c)^3$, where c is the sound speed and V is the volume involved. The total fusion rate becomes approximately

$$\gamma \approx \frac{12\sqrt{2}V}{8\pi^4 \omega_n c^3} \int_0^{\omega_{max}} \eta(\omega)\omega^4 d\omega \qquad (III.9)$$

Under the assumption that η is frequency independent, we obtain the following estimate for the total fusion rate

$$\gamma \approx \frac{60\sqrt{2}V\omega_{max}^5}{8\pi^4 \omega_n c^3} \qquad (III.10)$$

The maximum phonon frequency for a given fusion rate γ is found to be

$$\omega_{max} \approx [\frac{2\pi^4 c^3 \omega_n \gamma}{15\sqrt{2}\eta V}]^{1/5} \qquad (III.11)$$

If we adopt $c = 3 \times 10^5$ cm/sec, $\omega_n = 5.5$ MeV/$\hbar$, $\gamma = 10^{13}$ sec^{-1}, $V = 1$ cm^3 and $\eta = 10^6$, we then obtain $\omega_{max} = 1.8 \times 10^9$ radians/sec. From this exercise we speculate that if nuclear heat production is occuring in the cold fusion experiments, and if the heat is produced by a coherent mechanism similar to what is under discussion here, then the phonon modes which may be involved are in the 100-1000 Mhz spectral regime.

We should note at this point that neutron emission is fundamentally an incoherent process, and such reactions proceed with a rate which is proportional to the square of

the interaction matrix element. As a result, it is possible that the coherent reactions can dominate over the incoherent reactions by large factors. This will be discussed further elsewhere.

IV. The Interaction Matrix Element

We now must consider the computation of the interaction matrix element which couples a lattice state with n fusions and m phonons to another lattice state with $n \pm 1$ fusions and $m \pm 1$ phonons. There are two distinct mechanisms under consideration: an "electrostatic" mechanism for coherent fusion with single particle (^{3}He or ^{4}He) production, and a second "mechanical" mechanism for coherent fusion with two particle (p+t) production. The matrix elements for the two processes are based on the electromagnetic and strong force hamiltonians, respectively.

Associated with such a picture is a decomposition of the lattice hamiltonian into a piece which preserves fusion number n, and a second piece which does not. The hamiltonian in our model taken to be

$$H = \left[\sum_i \frac{\mathbf{p}_i^2}{2m_i} + \sum_{i<i'} V_{ii'}^{em} + \sum_{i<i'} V_{ii'}^{s} \right] \qquad (IV.1)$$

where V^{em} is the electromagnetic interaction, and V^s is the strong force interaction which we include between hydrogen isotopes. The decomposition which we require is

$$H = \Lambda_{nn} H \Lambda_{nn} + H_I \qquad (IV.2)$$

where the Λ_{nn} operator eliminates interactions which result in fusions, and the remaining part of the hamiltonian is denoted by H_I. We adopt the notation H_o to denote $\Lambda_{nn} H \Lambda_{nn}$.

The interaction matrix elements required for the cold fusion theory can be expressed in terms of the eigenfunctions of H_o, which obey

$$H_o \Phi_n(\Gamma) = E_n(\Gamma) \Phi_n(\Gamma) \qquad (IV.3)$$

These eigenfunctions are very complicated and cannot be used to numerically evaluate fusion rates except formally; our purpose here is to attempt to provide a formalism, and we will consider specific lattice models elsewhere. The total lattice wavefunction is then

$$\Psi = \sum_n \sum_\Gamma a_n(\Gamma) \Phi_n(\Gamma) \qquad (IV.4)$$

The complete interaction matrix element is given by

$$\langle \Psi_i \mid H_I \mid \Psi_f \rangle = \sum_n \sum_{\sigma=-1,1} \sum_{\Gamma} \sum_{\Gamma'} a_n^*(\Gamma) a_{n+\sigma}(\Gamma') \langle \Phi_n(\Gamma) \mid H_I \mid \Phi_{n+\sigma}(\Gamma') \rangle \qquad (IV.5)$$

The eigenstates denoted by Γ and Γ' differ differ by one phonon in an acoustical mode, although this concept is at this point not really very precise. Our picture is that the phonon structure of the lattice will be nearly identical in the case of a lattice with n fusions and with $n+1$ fusions, especially so in the case of acoustical phonons. It is in this spirit that we say that the states differ by a single phonon. A more precise way of counting the modes for a one dimensional lattice is to count nodes, and define the acoustical mode in question to be the one with the same number of nodes. It very likely that there will be many phonon modes which are involved in the fusion process simultaneously, and each of these will support coherent fusion chains independently of one another, similar to the situation of a multimode laser.

In principle, the phonon modes involved in this discussion may be optical phonon modes. Such a suggestion is motivated by noting that optical phonons involve relative nuclear motion which enhances the tunneling probability relative to acoustical phonons. The counter argument is that the interaction energy required to meet the criterion of section III is lower by three to five orders of magnitude for acoustical phonons.

V. Hartree Splitting

The interaction matrix element is proportional to the overlap integral of the two fusing nucleons, and as such is proportional to a tunneling factor (e^{-G}). It was observed earlier that one major difference between coherent fusion and incoherent (conventional) fusion was the dependence on the tunneling factor; incoherent fusion rates are proportional to the square of the matrix element (e^{-2G}). In this and following sections, we shall isolate this dependence through the use of a Hartree formulation.

Let the projected lattice hamiltonian be divided into three parts

$$H_o = H_A(\mathbf{r}_1, \mathbf{r}_2) + H_B(\mathbf{r}_3, \cdots, \mathbf{r}_N) + H_{AB} \tag{V.1}$$

where the two fusing nucleons have coordinates $\mathbf{r}_1$ and $\mathbf{r}_2$. The hamiltonian H_A includes effective inter-deuteron forces which account for electron correlations and a strong force potential. Within the Hartree framework, the eigenfunctions $\Phi_n(\Gamma)$ may be expressed as a product of a two-particle wavefunction and a many-particle wavefunction

$$\Phi_n(\Gamma) = \phi_n^A(\Gamma_A)\phi_n^B(\Gamma_B) \tag{V.2}$$

which satisfy Hartree equations

$$[H_A + \langle \phi_n^B(\Gamma_B) \mid H_{AB} \mid \phi_n^B(\Gamma_B) \rangle]\phi_n^A = \epsilon_n^A(\Gamma_A)\phi_n^A(\Gamma_A) \tag{V.3}$$

$$[H_B + \langle \phi_n^A(\Gamma_A) \mid H_{AB} \mid \phi_n^A(\Gamma_A) \rangle]\phi_n^B = \epsilon_n^B(\Gamma_B)\phi_n^B(\Gamma_B) \tag{V.4}$$

The interaction matrix element of (IV.5) can be written as

$$\langle \Psi_i \mid H_I \mid \Psi_f \rangle = \sum_n \sum_{\sigma=-1,1} \sum_{\Gamma} \sum_{\Gamma'} \sum_{kl} a_n^*(\Gamma) a_{n+\sigma}(\Gamma') \langle \phi_n^A(\Gamma_A)\phi_n^B(\Gamma_B) \mid H_I \mid \phi_{n+\sigma}^A(\Gamma'_A)\phi_{n+\sigma}^B(\Gamma'_B) \rangle_{kl} \tag{V.5}$$

where the indices k and l denote the nucleons which are undergoing fusion.

VI. The Electromagnetic Monopole Interaction

If we consider the reactions p + d $\rightarrow$ ^{3}He or d+d $\rightarrow$ ^{4}He, then the only terms in the interaction hamiltonian H_I which can provide the requisite coupling between the nuclear scale and the atomic scale are electromagnetic terms. The dominant coupling occurs through the E0 interaction; conventional d+d $\rightarrow$ ^{4}He + γ reactions at low energy proceed through E2 coupling to the the D-state admixture of the alpha wavefunction. But the virtual E0 interaction will be considerably stro[illegible]as it can be much longer range, and involves an overlap integral rather than a quadrupole matrix element.

The E0 coupling for a low energy photon comes about by taking the lowest order term in a series expansion of the coulomb interaction

$$\frac{e^2}{\mid \mathbf{r}_i - \mathbf{r}_j \mid} = e^2 \sum_\nu (2\nu+1) \frac{r_<^\nu}{r_>^{\nu+1}} P_\nu(\cos\theta_{ij}) \qquad (VI.1)$$

The monopole term is $e^2/r_>$. In the case of E0 coupling with a higher energy photon (coupling over distances of the order of a wavelength or larger), a more sophisticated version of the E0 operator is required. In this case the corresponding photon interaction becomes

$$\frac{e^2}{\mid \mathbf{r}_i - \mathbf{r}_j \mid} \rightarrow \frac{2e^2}{\pi} \int_0^\infty \frac{1}{\omega + k} \frac{\sin k \mid \mathbf{r}_i - \mathbf{r}_j \mid}{\mid \mathbf{r}_i - \mathbf{r}_j \mid} = \frac{2e^2}{\pi} \frac{f(\omega \mid \mathbf{r}_i - \mathbf{r}_j \mid)}{\mid \mathbf{r}_i - \mathbf{r}_j \mid} \qquad (VI.2)$$

where ω is the radial frequency of the exchanged photon, and where

$$f(z) = Ci(z)\sin(z) - si(z)\cos(z) \qquad (VI.3)$$

The multipole expansion of this more sophisticated photon operator is

$$\frac{2e^2}{\pi} \frac{f(\omega \mid \mathbf{r}_i - \mathbf{r}_j \mid)}{\mid \mathbf{r}_i - \mathbf{r}_j \mid} = \frac{2e^2}{\pi} \sum_{\nu=0}^\infty (2\nu+1) I_\nu(\omega, r_i, r_j) P_\nu(\cos\theta_{ij}) \qquad (VI.4)$$

where

$$I_\nu(\omega, r_1, r_2) = \int \frac{k}{\omega + k} j_\nu(kr_1) j_\nu(kr_2)\, dk \qquad (VI.5)$$

The E0 term is found to be

$$\frac{e^2}{r_>} \rightarrow \frac{2e^2}{\pi} I_0(\omega, r_i, r_j)$$

$$= \frac{e^2}{\pi} \frac{1}{\omega r_i r_j} \{\ln \frac{r_j + r_i}{r_j - r_i} + g[\omega(r_j + r_i)] - g[\omega(r_j - r_i)]\} \qquad (VI.6)$$

where

$$g(z) = -Ci(z) \cos z - si(z) \sin z \qquad (VI.7)$$

This more sophisticated version of the E0 operator behaves like $e^2/r_>$ over distances small compared to the wavelength of the exchanged photon, and goes as $2e^2/\pi\omega r_>^2$ in the limit that $r_>$ is much greater than a wavelength.

The wavelength of the exchanged photon can easily be on the order of the rod size or larger for coupling to acoustical phonons. Conversely, the interaction region is smaller in the case of coupling to optical phonons, since the the photon wavelength becomes significantly smaller than the rod dimensions.

VII. The Matrix Element for the Electromagnetic Coupling

Using the results of the last section for the E0 interaction, the matrix element of (V.5) becomes

$$\langle \Psi_i \mid H_I \mid \Psi_f \rangle = \frac{2e^2}{\pi} \sum_n \sum_{\sigma=-1,1} \sum_\Gamma \sum_{\Gamma'} \sum_j \sum_{kl} Z_j a_n^*(\Gamma) a_{n+\sigma}(\Gamma')$$
$$\langle \phi_n^A(\Gamma_A)\phi_n^B(\Gamma_B) \mid [I_0(\omega, \mid \mathbf{r}_k - \mathbf{r}_{kl}^{cm} \mid, \mid \mathbf{r}_j - \mathbf{r}_{kl}^{cm} \mid) + I_0(\omega, \mid \mathbf{r}_l - \mathbf{r}_{kl}^{cm} \mid, \mid \mathbf{r}_j - \mathbf{r}_{kl}^{cm} \mid)] \mid \phi_{n+\sigma}^A(\Gamma'_A)\phi_{n+\sigma}^B(\Gamma'_B)\rangle \qquad (VII.1)$$

where $\mathbf{r}_{kl}^{cm}$ is the center of mass coordinate for the pair of nucleons k and l. For the reactions p+d $\rightarrow$ ^{3}He and d+d $\rightarrow$ ^{4}He, the only contributions to the interaction matrix element come from regions of small (fermi) relative separation between the fusing nucleons. In this case, we can simplify (VII.1) somewhat to obtain

$$\langle \Psi_i \mid H_I \mid \Psi_f \rangle = \frac{4e^2}{\pi} \sum_n \sum_{\sigma=-1,1} \sum_\Gamma \sum_{\Gamma'} \sum_j \sum_{kl} Z_j a_n^*(\Gamma) a_{n+\sigma}(\Gamma')$$
$$\langle \phi_n^A(\Gamma_A) \mid \phi_{n+\sigma}^A(\Gamma'_A)\rangle \langle \phi_n^B(\Gamma_B) \mid I_0(\omega, 0, \mid \mathbf{r}_j - \mathbf{r}_{kl}^{cm} \mid)] \mid \phi_{n+\sigma}^B(\Gamma'_B)\rangle \qquad (VII.2)$$

where we have assumed that most of the contribution from the E0 coupling comes from long range interactions (so that $\mathbf{r}_{kl}^{cm}$ can be taken to be moderately well-defined in the argument of I_0). In this equation we have achieved an explicit separation between the nuclear part (the overlap integral of the nucleons) and the lattice part of the interaction matrix element.

The overlap integral includes the tunneling probability, and can in principle be computed with relative ease from a two particle Hartree calculation. There is some information concerning the magnitude of the overlap integral from calculations in the literature. For example, the decay rate for D_2 decay to ^{3}He+n of [28] can be used to find $\langle \phi_n^A(\Gamma_A) \mid \phi_{n+\sigma}^A(\Gamma'_A)\rangle \approx 2 \times 10^{-43}$. Leggett and Baym have given a rigorous upper limit for D_2 decay in metals, and this can be used to get an upper bound of $\langle \phi_n^A(\Gamma_A) \mid \phi_{n+\sigma}^A(\Gamma'_A)\rangle \approx 6 \times 10^{-35}$. The claimed experimental observations of neutrons would yield an overlap integral of about 10^{-23} for a neutron emission rate of 10^{-24} sec^{-1} per D_2 pair. The overlap for a ground state HD molecule is roughly 3×10^{-36}.

The computation of the piece of the interaction matrix element which includes the lattice part of the coulomb interaction requires some effort. As an extremely crude ballpark estimate of the term, one might imagine that it would be on the order of the product of the number of s-wave deuteron pairs in the lattice which have large tunneling probability, the number of excess charges accessible within a wavelength of the exchanged photon, e^2 and some characteristic inverse distance between the fusing nucleons and the excess charge.

It appears possible that in regions where numerc. e. 10^{23}) pd pairs occur with substantial (although not prohibited) overlap integrals, and in the vicinity ($10^{-2} - 10^{-1}$ cm) of a very large amount of excess charge (10^{15} excess charges), that the interaction matrix element has the possibility of being large enough to exceed the phonon energy of some acoustical phonon modes. The p + d $\rightarrow$ ^{3}He reaction would have a much larger interaction matrix element due to the smaller reduced mass of the proton/deuteron system than the interaction matrix element for the d+d $\rightarrow$ ^{4}He reaction, and as a result would be more likely to occur in a system where both protons and deuterons are present.

A scenario which is consistent with this model is one where p+d reactions occur at relatively low phonon frequency initially, and as the presence of phonons improves the overlap integrals, the reactions increase. In some lattices, the phonon population might become large enough to begin driving the d+d $\rightarrow$ ^4He and d+d $\rightarrow$ p+t reactions, with the associated increase in phonon emission.

VII. Summary and Discussion

We have proposed a model for coherent fusion in which the nuclear energy is released into the acoustical phonon modes of a lattice. We have argued that the coupling of the nuclear energy to the phonons can occur rapidly if the interaction couples energy one phonon at a time with an interaction energy which exceeds the phonon energy.

The new coherent reactions p+d $\rightarrow$ ^{3}He + ΔE(lattice) and d+d $\rightarrow$ ^{4}He + ΔE(lattice) can proceed through electromagnetic E0 coupling of the nucleons to excess charge in the lattice, and we have provided an approach with which to address the computation of the interaction matrix element.

We postulate that the reaction d+d $\rightarrow$ p+t + ΔE(lattice) can also proceed coherently, and that it can be calculated by evaluating equation (IV.5) using the strong force terms in H_I. The strong force is a short range force and cannot by itself couple to the rest of the lattice. In this case, the generation of a phonon would have to occur as a secondary process, brought about through the differences in lattice interactions with d+d versus p+t. For example, a substitution of p + t for d+d will alter the phonon spectrum through differences in local masses. The forces felt by nearby nucleons due to a proton will be larger than that for a deuteron due to the larger zero point energy of the proton, and this may provide a secondary mechanism for phonon geneation.

Reactions mediated by the strong force do not require excess charge to be present. As a result, we consider the p+d $\rightarrow$ ^{3}He and d+d $\rightarrow$ ^{4}He reactions to be "electrical" coherent reactions, and the d+d $\rightarrow$ p+t reaction to be a "mechanical" coherent reaction, if the occur in the manner outlined in this paper.

As a result of the arguments which have been given in this work, we propose that three new coherent reactions may proceed by two distinct mechanisms. Of these reactions, the p+d reaction should be the easiest to exploit because of the smaller reduced mass (and hence larger tunneling probability). The coherent d+d $\rightarrow$ p+t reaction may be responsible for the reported observation of tritium production in electrolysis cells. The ^{4}He branch is probably the most difficult to observe at large rate, but would have the best energy production per unit mass (fuel) ratio.

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Enclosure

Searches for low-temperature nuclear fusion of deuterium in palladium

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A series of experiments has been performed to determine whether nuclear fusion processes occur in palladium rods that have been electrochemically charged with deuterium. With a variety of metallurgical pretreatment procedures and different electrolytes, no evidence has been obtained for any excess enthalpy, neutron, gamma ray, tritium or helium production during electrolysis of D_2O with palladium cathodes.

IN response to claims of unusual nuclear processes occurring at or near ambient temperature and pressure[1], we report the results of a detailed series of experiments on the behaviour of palladium rods that have been electrochemically charged with deuterium. Employing a series of metallurgical pretreatment procedures and several different electrolytes, we have not found evidence for any excess enthalpy, neutrons, γ-rays, helium or tritium. Experiments performed with sensitive neutron and γ-ray detectors have allowed us to place stringent upper bounds on the rate of nuclear fusion events occurring in these systems.

Preparation of electrolytic cells

We used several different sources of Pd in these experiments: 2.2-mm- and 3.8-mm-diameter cold-worked Pd rods and Pd sponge (>99.9% Pd) were obtained from David Fell, Inc. (Los Angeles); 1.0-mm-diameter cold-worked Pd rods (>99.9% Pd) were obtained from AT&T Bell Laboratories and from Aesar, Inc.; 0.25-mm-diameter wire (>99.99% Pd) was obtained from Engelhard Industries; a Pd ellipsoid, having a major axis of 3 cm and minor axes of 0.7 cm and ~0.44 cm respectively, was obtained by r.f. melting of a Pd ingot (>99.999% Pd) in an argon atmosphere. Portions of the 1.0-mm- and 2.2-mm-diameter rods were recast by melting the Pd at 1,600 °C for 5 min with either unregulated cooling to ambient temperature or by cooling at a rate of 50 °C min^{-1} through the melting point. The recasting procedures were performed in Al_2O_3 tubes under 1 atm of CO(g) and yielded Pd rods of 2.1-mm- and 2.5-mm-diameter, respectively. In addition, the Pd sponge material was formed into 2.5-mm rods, after melting with an acetylene torch, by centrifugal casting. All cast rods were smoothed with sandpaper and ultrasonically cleaned in D_2O before use in the electrochemical cell.

We made a stock solution of 1.0M LiOD by adding a weighed amount of Li wire to 99.5% D_2O (Aldrich); immediately before use we diluted this stock solution to 0.1M LiOD with D_2O. Similarly, we prepared 0.1M LiOH from Li wire and deionized H_2O. In some cases, we pre-electrolysed the solutions before use, using Pt working and counter electrodes[2]. The electrochemical cells for neutron, γ-ray and tritium measurements were typically 20-cm^3 glass reaction vessels fitted with polypropylene caps to prevent exchange of D_2O with ambient H_2O. The caps also had a small vent for the evolving gases, and the electrode leads were sealed with epoxy resin to the cap of the vessel. The extent of H/D exchange in the D_2O electrolyte was measured by ^1H NMR integration of the H_2O peak relative to a benzoic acid standard; in a typical electrolysis cell, the D_2O was found to contain 0.8 atom% ^1H after 540 h of operation.

The Pd rods were generally placed in the centre of the glass reaction vessel and were surrounded by a Pt-foil, wire or gauze counter electrode. A cathodic current was maintained from initiation until the end of all experiments to avoid outgassing of the D from the Pd rod. The potential of the Pd cathode during electrolysis was measured relative to a Pd-wire reference electrode charged with D to the α-β phase transition[3]; generally the Pd rods were charged to -1.5 V relative to this reference potential (see below) at current densities of 64 mA cm^{-2}, and the resistance-corrected charging potential was ~ -0.8 V relative to the Pd/D reference. A charged specimen (0.025-cm-diameter × 2.3-cm-length rod, charged at 75 mA cm^{-2} for >12 h) had an open-circuit potential of < -0.60 V relative to a Pd/D reference electrode, as determined by current-interrupt methods[4] (5.0 ms after current interruption). Measurements of the volume of D_2 gas liberated from three separate electrochemically charged D/Pd samples (each 0.25-cm diameter × 2.5-cm length) were made by heating them with a propane torch, in a quartz tube preloaded with Ar, until gas evolution ceased. The D/Pd stoichiometries of 0.77, 0.79 and 0.80 obtained from these measurements were taken to be representative of the D/Pd stoichiometry for the charged cathodes used in this work. X-ray photoelectron spectroscopic (XPS) analysis of a 0.25 × 2.5-cm electrode, after two weeks under constant-current electrolysis conditions in 0.1M LiOD/D_2O, showed the presence of Pd and no signal above background in the Li region (that is, less than monolayer equivalents of Li in the 30-Å escape depth of the photoemitted electrons). We subjected samples to a variety of pretreatments, including recasting, vacuum annealing and anodization[5]. Also, current levels were varied to include oscillating-current pulses, abrupt current steps and/or constant currents for the duration of an experiment.

Neutron measurements

Table 1 lists the known exoergic nuclear reactions of deuterium (^2H) with all stable or long-lived isotopes of hydrogen, helium and lithium, together with the energies released in the reactions. At low laboratory energies (a few keV), reactions (b) and (c) occur with about equal probability and with $\sim 10^7$ times the probability of reaction (d)[6]. Reaction (e) occurs with $\sim 10^5$ times the yield of (f). At the much lower energies relevant to the present work at ambient temperature, interaction-barrier considerations lead one to expect that reaction (a) should occur with the highest probability[7], followed by reactions (b) and (c) (about equal probabilities), then (e), (f), (g), (h), (i), (j) (k) and

(l), in order of decreasing probability. Table 1 also lists the number of reactions per second that would be needed to produce an output power of 1 W for each of the reactions under consideration.

The neutron detector used in our measurements has been recently developed[8], and is somewhat similar to that described in ref. 9. It consists of a 40-cm cube of polyethylene with a 10×10-cm open horizontal channel through the centre, in which the electrolysis cells were placed. Thermalized neutrons were detected with twelve ^{3}He proportional counters embedded in the cube around the central channel. The cube was surrounded by a 0.6-mm-thick cadmium layer, a 10-cm-thick passive polyethylene shield, a 2π array of 2.5-cm-thick plastic scintillator paddles and a 25-cm-thick passive paraffin-wax shield. In some experiments, we used only 8 counters (14% efficiency), whereas in other experiments we used as many as 11 counters (20% efficiency); to account for this variation, we made appropriate calibrations and foreground/background comparisons for each run. Software cuts in the pulse height spectra were made to reject small signals from γ-rays and preamp noise, and higher energy α-particles. Further cuts were made on the timing spectra between the paddles and counters to reject cosmic-muon-induced neutron production inside the cube. The remaining background rate of 80–120 counts h^{-1} was due to roughly equal contributions from alpha emission from the counter walls and from high-energy neutrons produced by cosmic rays outside the cube.

Table 2 summarizes a series of experiments designed to detect the emission of neutrons from the samples. In no case did we observe a statistically significant neutron count that exceeded the background level. Figure 1 displays the neutron detection rate as a function of time for a representative counting period. In this particular series of runs, background runs with dummy cells were interspersed between foreground runs for four fully charged cells. All runs depicted in Fig. 1, as well as all other runs performed, agreed with the average rate of ~610 neutrons per hour, within the combined statistical and systematic errors. We have thus established an upper limit on the net neutron rate (that is, foreground minus background) of 100 neutrons per hour at the 95% confidence level. For the 0.22×10-cm Pd rod, this corresponds to an upper limit of 0.070 neutrons per cm^3 Pd per s, which contrasts with the specific neutron flux of 3.2×10^4 neutrons per cm^3 Pd per s reported in previous work[1]. For a D/Pd ratio of 0.80, the 1σ upper bound of 73 neutrons per cm^3 Pd per h for the 0.44×3-cm Pd ellipsoid (Table 2) corresponds to an upper limit, at the 95%-confidence level, of 1.5×10^{-24} D(d, n) fusions per d-d pair per second.

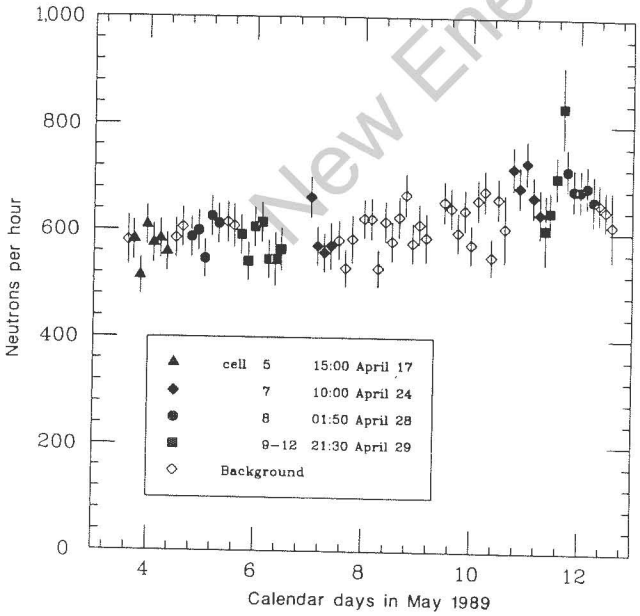

FIG. 1 Neutron rate for various runs with and without electrolysis cells. For each run, the data shown are the detector counts, divided by the efficiency, and the run duration (~3 h). The errors include only the statistical uncertainty; an estimate of the systematic uncertainty was calculated by determining the excess variance among background runs. The times in the inset refer to the initial application of cathodic current to each cell, and the current was maintained continuously through the analysis period.

TABLE 1 Low-energy deuteron fusion reactions

	Reaction	Energy release (MeV)	Reactions per second per 1 W output
a	$^1H + ^2H \rightarrow ^3He + \gamma$	5.5	1.1×10^{12}
b	$^2H + ^2H \rightarrow ^3He + n$	3.3	1.9×10^{12}
c	$^2H + ^2H \rightarrow ^3H + ^1H$	4.0	1.6×10^{12}
d	$^2H + ^2H \rightarrow ^4He + \gamma$	23.8	2.6×10^{11}
e	$^2H + ^3H \rightarrow ^4He + n$	17.6	3.6×10^{11}
f	$^2H + ^3H \rightarrow ^5He + \gamma$	16.7	3.7×10^{11}
g	$^2H + ^3He \rightarrow ^4He + ^1H$	18.4	3.4×10^{11}
h	$^2H + ^4He \rightarrow ^6Li + \gamma$	1.5	4.2×10^{12}
i	$^2H + ^6Li \rightarrow 2^4He$	22.4	2.8×10^{11}
j	$^2H + ^6Li \rightarrow ^7Li + ^1H$	5.0	1.3×10^{12}
k	$^2H + ^6Li \rightarrow ^7Be + n$	3.4	1.8×10^{12}
l	$^2H + ^7Li \rightarrow 2^4He + n$	15.1	4.1×10^{11}

Gamma ray measurements

We detected γ-rays using a high-purity intrinsic-germanium detector (150 cm^3) and either a 7.6-cm-diameter × 7.6-cm-length NaI scintillator or a $15 \times 15 \times 25$-cm NaI scintillator. The detectors and source cells were shielded by 10 cm of lead. In the later runs, a plastic scintillator paddle was used to reject muon-coincident events.

Table 2 presents the results of various runs; no statistically significant evidence was obtained for γ-ray production from any of the electrolysis cells. The entire γ-energy foreground spectrum from 30 keV to 6 MeV is displayed in Fig. 2; for all of the Pd electrolysis cells, both the foreground and background spectra were virtually identical to this spectrum. Our sensitivity places a conservative upper limit of <200 fusions per hour from reaction (a) because of the absence of the expected peaks from a 5.5-MeV γ-ray. Also, no γ-rays at 23.8 MeV have been seen in long-duration NaI-detector runs (Table 2). Furthermore, we have observed no excess bremsstrahlung or annihilation radiation which would be expected from reaction (d) if 23.8-MeV e^+e^- pairs were emitted[10] instead of 23.8-MeV γ-rays. The Ge-detector gamma-ray spectra also showed no evidence for γ-ray lines from the Coulomb excitation of the various Pd isotopes by protons and/or α-particles from reactions (c), (e), (g), (i), (j) or (l).

We also note that an authentic 2.223-MeV γ-ray signal originating from thermal-neutron capture in hydrogen (2 kHz ^{252}Cf source placed in a H_2O bath) and observed with a 7.6 × 7.6-cm NaI detector has a much larger width (as a result of the intrinsic instrumental resolution limitation of NaI) than the γ-ray spectrum presented in previous work[1], and also displays a Compton edge just below the full-energy (2.223 MeV) peak that is missing in the published spectrum. For these reasons, we can conclude that the published peak[1] does not arise from thermalized neutrons. During the course of our studies, an independent analysis of the γ-ray peak was performed by Petrasso *et al.*[11], who reached similar conclusions.

If the known $^2H + ^2H$ reactions (b), (c) and (d) were to generate 10 W of power[1], with their known relative yields, $\sim 8 \times 10^{12}$ neutrons s^{-1}, $\sim 8 \times 10^{12}$ tritium atoms s^{-1} and $\sim 8 \times 10^5$ 23.8-MeV-γ-rays s^{-1} should be produced, respectively (see Table 1). Similarly, 10 W of power generated by reaction (a) should produce $\sim 10^{13}$ 5.5-MeV-γ-rays s^{-1}. Alternatively, 10 W of power

TABLE 2 Summary of neutron and photon detection experiments

Cell no.	Cathode dimensions* (cm)	Comments	Electrolyte	t_a† (h)	γ Detection‡ Start§ (h)	γ Detection‡ upper limit‖ 5.5 MeV (γ per cm³ Pd per h)	γ Detection‡ upper limit‖ 23.8 MeV (γ per cm³ Pd per h)	n Detection Start § (h)	n Detection upper limit‖ (n per cm³ Pd per h)
1	0.0254×0.2	99.99% Pd¶ no high I or redox activation	1.0M LiOH, D_2O	0.45	0#	**	**	—	—
2	0.0254×0.2	99.99% Pd¶ no act.	1.0M LiOH 50:50, D_2O:H_2O	0.45	0#	2.2×10^6	—	—	—
3	0.0254×8	99.99% Pd¶ 300 °C, 10^{-6} torr 2 h	0.1M LiOD, D_2O	0.45	—	—	—	0	2.9×10^4
4	hollow cylinder o.d.=0.52, i.d.=0.47 l=2.5	>99.9% Pd†† no act.	0.1M LiOD, D_2O	10	0	620	—	51	820
5	0.1×3.7	99.99% Pd‡‡ 350 °C, Ar 3 h	0.1M LiOD, D_2O	6.9	14	4,700	4.2×10^5	0	3,200
					204	3,000	4.0×10^5	38	2,800
								93	3,600
								465	2,900
		pulsed electrolysis§§			570	1,100	—	559	4,700
6	0.22×10	>99.9% Pd‖‖ 300 °C, 10^{-6} torr 2 h	0.1M LiOD, D_2O	33.6	0	350	2.9×10^4	34	210
					86	240	3.0×10^4	134	200
					258	470	3.0×10^4	278	180
					421	140	3.4×10^4		
					627¶¶	220	1.3×10^4		
7	0.25×0.8	>99.99% Pd cast## no act.	0.1M LiOD, D_2O	43.4	33	1,800	2.9×10^5	223	1,700
					193	1,500	2.9×10^5		
					291¶¶	3,200	7.4×10^4		
8	~0.44×0.7×2.95	>99.999% Pd remelted*** 300 °C, 10^{-6} torr 3 h	0.1M LiOD, D_2O	~130	36	70	1.2×10^4	161	73
					180¶¶	85	3.3×10^3	328	110
9–12	0.25×2.5	>99.9% Pd cast††† 300 °C, 10^{-6} torr 12 h	0.1M LiOD, D_2O	43.4	131¶¶	420	3.5×10^3	140	130
		stepped current‡‡‡			227¶¶	160	3.5×10^3	158	200

* Cathodes were rods except where otherwise noted. Dimensions indicated are: diameter × length.

† The activation time, $t_a = r^2/D$, where r is the radius of the cathode and $D = 10^{-7}$ cm² s^{-1}.

‡ Photon counting experiments were performed using a Ge detector and a 7.6×7.6-cm NaI scintillation detector in parallel, except where otherwise noted.

§ Time in hours at which either photon or neutron counting was initiated, measured from the initiation time of the cell. Typical durations for photon and neutron counting sessions were ~6 h.

‖ The estimated upper limit for neutron or photon flux from each cell expressed as the flux per unit Pd-cathode volume: For $n_f > n_b$, flux = $[(n_f - n_b) + (\sigma_f^2 + \sigma_b^2 + \sigma_{sys}^2)^{0.5}]/(\varepsilon V_c)$; for $n_f < n_b$, flux = $(\sigma_f^2 + \sigma_b^2 + \sigma_{sys}^2)^{0.5}/(\varepsilon V_0)$, where n_f and n_b are the foreground and background count totals, σ_f and σ_b are the respective standard deviations, σ_{sys} is the estimated systematic error, ε is the detector efficiency and V_c is the volume of the Pd cathode. $\sigma_{sys}/n_f \approx 10\%$ (neutrons); <6% (photons).

¶ Englehard Inc.

Only the Ge detector was used for these experiments.

** Exact estimate of upper limit for these runs is unavailable, but approximates that for subsequent photon counting runs.

†† Manufacturer unknown; the composition of the cathode was assayed using energy dispersive X-ray analysis.

‡‡ Johnson Mathey Inc.

§§ The current density of the cathode was pulsed between 70 mA cm^{-2} and 350 mA cm^{-2} at 1 Hz.

‖‖ David Fell Co.

¶¶ A 15×15×25-cm NaI scintillation detector was employed in parallel with a Ge detector.

Pd rod cast from a melt of >99.99% Pd which was solidified at a cooling rate of 50 °C min^{-1} through T_m.

*** Irregularly shaped Pd ingot was formed by remelting of 99.999% Pd and solidified at a cooling rate of >200 °C min^{-1} through T_m.

††† Centrifugal casting was employed to prepare rods from Pd obtained from '‖‖' above.

Cells 9–12 were photon and neutron counted simultaneously.

‡‡‡ The current density was stepped from 64 mA cm^{-2} to 320 mA cm^{-2} immediately before the foreground neutron count.

from reaction (e) would produce $\sim 4\times10^{12}$ neutrons s^{-1}. If the energy for reaction (d) could be converted to phonons by some unknown mechanism, as has been suggested[12], the coupling between the excited ^{4}He compound nucleus and the lattice would have to be electromagnetic. The same electromagnetic field of the excited ^{4}He nucleus must also cause the emission of γ-rays and e^+-e^- pairs, which would have been detected in our experiments. Although reaction (i) would not necessarily yield external radiation, the small difference in reduced masses between $^2H + {}^6Li$ and $^2H + {}^7Li$ makes it extremely unlikely that reaction (l) would be totally suppressed and neutrons would be detected as the product of $^2H + {}^7Li$ fusion events. Our measurements therefore force us to conclude that none of these known nuclear fusion reactions is occurring at a substantial rate (conservatively, <100 per s per cm³ Pd) under our experimental conditions.

Tritium measurements

We have also analysed the electrolyte for production of excess ^3H that might result from reaction (c) in Table 1. Tritium measurements were performed on a Beckman Instruments LS-5000TD scintillation counter using a Beckman Ready-Gel scintillation 'cocktail'. The background tritium level of our D_2O feedstocks was 148 ± 12.2 decays min^{-1} ml^{-1} (d.p.m. ml^{-1}). Electrolysis at 64 mA cm^{-2} of a 0.22-cm-diameter × 10-cm-length Pd rod for 458 h yielded a final ^3H activity of 162.3 ± 12.7 d.p.m. ml^{-1}, whereas a separate run with a 0.25-cm-diameter × 0.8-cm-length rod for 345 h at 64 mA cm^{-2} yielded a final ^3H activity of 149.5 ± 12.2 d.p.m. ml^{-1}. Some increase in the ^3H level of the electrolyte would be expected due to the isotope separation factor of $^3H/^2H$ at Pd, but this ratio is evidently sufficiently close to unity in 0.1M $LiOD/D_2O$ that little detectable ^3H buildup occurs over the time periods of our experiments. The specific tritium production rate of $(5 \pm 7) \times 10^3$ ^3H per cm^3 Pd per s observed for the 0.22 × 10-cm rod is far lower than the specific rate of $1-2 \times 10^5$ ^3H per cm^3 Pd per s reported in previous work[1]. Preliminary results reported by other groups have detected larger levels of ^3H from Pd cathodes in D_2O (ref. 12), but we note that scintillation-cocktail chemiluminescence of unneutralized basic solutions might be responsible for these results.

He measurements

Results claiming high ^4He content (ref. 13; B. S. Pons on 'Science Journal', Public Broadcasting Network, United States and various newspaper publications) in the evolved gases at Pd cathodes have prompted us to investigate the composition of the gases in the headspace of our electrolysis cells. The analysis was performed with a VG model 7070 E magnetic-sector mass spectrometer. The instrument was operated in the electron-ionization mode (70 eV) at a mass resolution of 1,000. The gas mixtures that we used as calibration standards were D_2/O_2 (2:1), D_2/O_2 (2:1) containing 1,100 p.p.m. ^4He and D_2/O_2 (2:1) containing 8 p.p.m. ^4He. Samples of 3.0 ml (at a nominal pressure of 1 atm) from these calibration mixtures, from a sample of the gas in the headspace of a Pd/D_2O cell (0.2-cm-diameter × 10-cm-length Pd rod) that had been operating at 64 mA cm^{-2} for >300 h, and from a sample of the ambient air in the laboratory, were injected using a gas-tight syringe for analysis. We averaged together sixty 2.5-s scans from mass 0–60 for each run.

When care was taken to exclude laboratory air from the gas samples of the Pd/D_2O headspace, no He was observed above the instrumental background (<1 p.p.m. relative to total gas content of 2:1 D_2/O_2). We used the Ar^+ signal at $m/e^- =$ 40 a.m.u. as an indication of the amount of air in these samples; the relatively intensity of the Ar^+ to $^4He^+$ peak ranged from 2,000–4,000 in the ambient air. A quantitative analysis indicated that the ambient air in our laboratory contained 4 ± 1 p.p.m. ^4He. We also note that the lifetime of He inside $Pd/D_{0.65}$ samples is known to be >12 yr (R. C. Bowman Jr, personal communication and ref. 14); thus, our inability to detect He in the headspace is in accord with previous work. We analysed the He content of a 0.25-cm-diameter × 2.5-cm-length Pd rod (sample no. 11 in Table 2, which had been used in 0.1M $LiOD/D_2O$ for 158 h at 64 mA cm^{-2}, followed by 4 h at 320 mA cm^{-2}, followed by 169 h at 64 mA cm^{-2}) by melting the Pd and performing a mass spectroscopic analysis on the evolved gas. This measurement indicated no detectable ^3He or ^4He, above the instrumental background ($<8 \times 10^{11}$ He per cm^3 Pd). We also note that the previously claimed levels of ^4He (1–10 p.p.m. relative to D_2 for a 0.10 × 10-cm rod at 64 mA cm^{-2} current density (C. Walling, personal communication and ref. 12)) are 30–300 times the expected ^4He yield from the reaction $^2H + ^2H \rightarrow ^4He + 23.8$ MeV for the reported excess power of 0.079 W (ref. 1).

Excess enthalpy measurements

We have also performed calorimetric experiments to determine the heat produced during electrolysis of D_2O and H_2O at Pd. The measurements were performed using the isoperibolic calorimetry mode (a constant-temperature system with a well defined heat loss rate to a constant-temperature bath) of a Tronac, Inc. (Orem, Utah) Model 1250 calorimeter equipped with a 60 l bath. Some experiments were also performed in a 5 l constant-temperature unit using a 30-cm^3 interior-volume dewar flask, with the dewar walls containing 1 atm of air. The dewar held, typically, 30–50 ml of electrolyte and was fitted with a small vent for evolved gases and had sealed inlets for the cell leads. The temperatures of the baths were maintained by active heating/cooling feedback at 27.00 ± 0.01 °C (60 l bath) and 25.00 ± 0.01 °C (5 l bath), and the temperatures of the electrolysis cells were monitored relative to the thermocouples immersed in the constant-temperature baths. The electrolyte was vigorously stirred with a motor; the stirring rate was maintained as constant as possible and the Joule heating as a result of stirring the

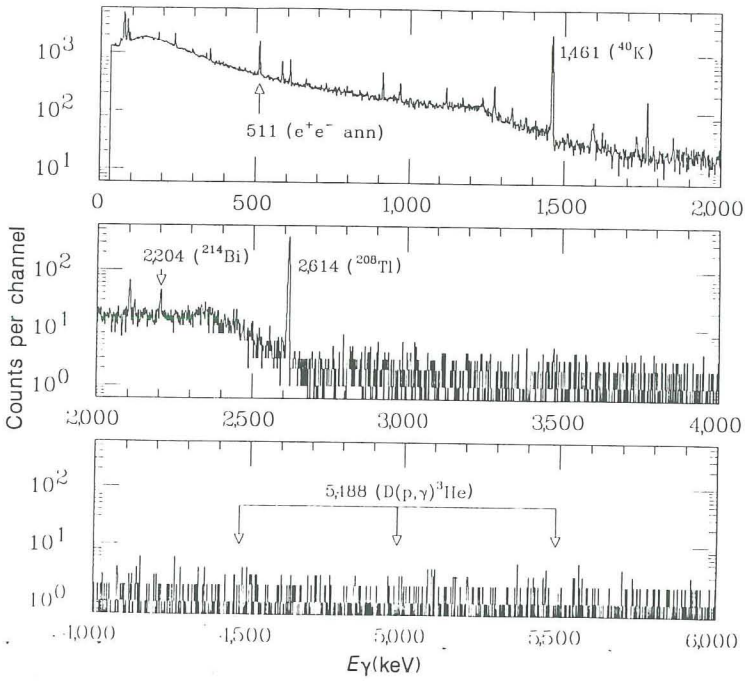

FIG. 2 Gamma-ray spectrum obtained from cell 5 in Table 2 using a Ge detector. Each channel has a width of 1.84 keV. The current was pulsed at 1 Hz from 70–350 mA cm^{-2}; the foreground analysis time was 41 h. All γ-ray lines correspond to well-known transitions in the ^{238}U and ^{232}Th decay chains and ^{40}K; no differences between foreground and background spectra were found for this run or for any other γ-ray run in Table 2. Major lines in the spectrum are labelled; also noted are the locations of recoil-corrected full-energy, first-escape and second-escape peaks from the γ-ray in reaction (a), Table 1. An energy window which includes these peaks has an efficiency of 2.9% in our geometry.

TABLE 3 Calorimetric data

(a) *Cast Pd rod* (0.21 × 2.1 *cm*), 0.1*M LiOD*/D_2O

	Time (h)	Current density (mA cm^{-2})	Electrolysis power (W)*	Resistor power (W)	Total power (W)†	Temperature, T_{cell} (°C)‡	Heating coefficient (°C/W)§
A	14.7	108	0.463	0	0.463	31.80	14.0
	16.0	74	0.255	0.212	0.467	31.82	14.0
B	63.7	74	0.231	0.211	0.442	32.04	15.2
	66.0	110	0.429	0	0.429	32.01	15.6
C	88.7	110	0.407	0.212	0.619	34.69	15.2
	94.5	140	0.607	0	0.607	34.64	15.4
D	113.2	72	0.222	0.211	0.433	32.13	15.8
	115.0	108	0.426	0	0.426	32.08	15.9
E	161.0	140	0.595	0	0.595	34.69	15.8
	164.5	115	0.436	0.164	0.600	34.71	15.7

(b) *Drawn Pd rod* (0.38 × 2.4 *cm*), 0.1*M LiOH*/H_2O

Time (h)	Current density (mA cm^{-2})	Total power (W)‖	Electrolysis power (W)¶	Heating coefficient (°C/W)#	ΔT_{th} (°C)**	$\Delta T'_{th}$ (°C)††	ΔT_{meas} (°C)‡‡
3.6	8	0.064	0.029	12.6 ± 0.3§§	0.81 ± 0.02	0.37 ± 0.02	0.35 ± 0.05
20.7	60	0.668	0.405	11.7	7.8	4.7	4.67 ± 0.05
47.5	60	0.646	0.384	13.1 ± 0.3§§	8.5 ± 0.3	5.0 ± 0.2	5.45 ± 0.05

* Electrolysis power = (E − 1.54) × I.
† Total power = electrolysis power + resistor power.
‡ Bath temperature = 25.00 ± 0.02 °C; temperature rise in cell as a result of stirring is 0.30 ± 0.05 °C.
§ Heating coefficient = [T_{cell} − (25.00 + 0.30)]/Total power.
‖ Total power = E × I.
¶ Electrolysis power = (E − 1.48) × I.
# Heating coefficients determined at the given electrolysis power, obtained from calibrations with a resistor using: heating coefficient = ΔT/(resistor power).
** ΔT_{th} = heating coefficient × total power.
†† $\Delta T'_{th}$ = heating coefficient × electrolysis power.
‡‡ $\Delta T_{meas} = T_{cell} - (T_{bath} + T_{stirrer})$; $T_{bath} + T_{stirrer}$ = 25.30 ± 0.05 °C.
§§ From three determinations.

solution was measured (~0.3 °C) and accounted for in all measurements. In the calorimetry measurements, a fixed power was applied to a resistive load that was included in the cell, while a known power (at constant current density, 64 mA cm^{-2}) was simultaneously applied to the Pd/Pt circuit. This condition was maintained until the temperature of the cell reached a constant value T_{cell}; then the power through the Pd/Pt circuit was changed to a new steady-state value. In response to this perturbation in the steady-state condition, the power in the resistor was then adjusted to maintain the cell temperature at T_{cell}. The ratio between the power input into the Pd/Pt circuit and the power input in the resistive load then allowed accurate determination of the steady-state power production from the Pd/Pt electrolysis circuit. Experiments were performed for both increases and decreases in the current density of the Pd/Pt system. An additional calibration of the rate of heat loss from the dewar was obtained by varying the resistor power at a constant Pd/Pt current density. This latter method was used to calculate the heat balance for the Pd/0.1M LiOH-H_2O/Pt control cell.

Table 3 summarizes the results of these calorimetry experiments. Cells operated in H_2O or D_2O electrolytes showed less heat produced in the electrolyte than power input into the Pd/Pt circuit. This is consistent with the expectation for an electrolysis reaction in which some of the input power will be carried away in the enthalpy of the D_2 (or H_2) and O_2 gases escaping from the measurement system. A correction for the enthalpy contained in the escaped D_2 and O_2 gases can be conveniently performed by subtracting 1.54 V for D_2O (1.48 for H_2O) from the applied voltage[15]; $V' = V - 1.54$ is therefore the amount of voltage that will be effective in heating the contents of the D_2O electrolysis cell[16]. Applying this correction, we obtained agreement between power input in the Pd/Pt circuit and power production in the electrolysis cell to better than 6%, which is in excellent agreement with expectations based on conventional chemical reaction processes for the electrolysis of water. The small excess power production over the expected lower limit is in accord with previous studies that have identified a current-density-dependent H_2-O_2 recombination process as the cause of some heat production above the value expected in the absence of any recombination[17].

We have also identified several subtle sources of possible error in the calorimetric measurements. If power was applied only to the load resistor until T_{cell} was attained and then the Pd/Pt circuit was turned on and the load resistor turned off, incorrect estimates of Pd/Pt power production were obtained from the new steady-state temperature reading. This resulted from changes in the heat losses when the Pd/Pt cell was being charged, which in turn resulted in changes of the Newton's cooling constant, as seen in Fig. 3. Errors of ±50% in the Tronac, Inc. vacuum-jacketed calorimeter dewar could be obtained in this fashion, whereas smaller errors were observed in the air-jacketed dewar. We also sometimes observed abrupt or gradual changes in the rate of heat loss from these cells, presumably resulting from a change in the rate and/or form of gas evolution[18]. These changes often resulted in a sustained temperature rise of the cell (which might be interpreted in terms of the onset of excess enthalpy production), but recalibration with the load-resistor method during this period showed no evidence for any anomalous power production, even after the reported activation period for the Pd rods[1,19] had been exceeded.

We also observed that even in the presence of vigorous evolution of gas bubbles at current densities >100 mA cm^{-2}, temperature gradients existed in cells that did not have efficient mechanical stirring. With power applied simultaneously to the resistive load and the Pd/Pt circuit, we found a temperature differential of up to 2 °C in an unstirred operating cell (6–8 °C above ambient) of dimensions very similar to those used in

previous work[1]. This differential exists because of inadequate mixing of the various sources of heat in the cell, including the heat of neutralization at the anode, the resistive heating in the cathode–anode gap and the heating from the calibration load resistor. This error could only be eliminated by efficient mechanical stirring of the cell; thus, calorimetric measurements and calibrations performed in unstirred solutions, even in the presence of gas evolution, may result in errors in enthalpy measurements. We have also observed that the apparent Newton's cooling constant for the above cell was a complicated function of the current density applied to the Pd cathode (Fig. 3) and of the position of the thermistor relative to the resistor; failure to account for changes of this constant led to systematic errors which resulted in errors in the amount of power produced by the electrolysis circuit. We note that, without a total heat-flow balance in each electrolysis cell, the observation of a small temperature differential betweeen a Pd/D_2O cell and a Pd/H_2O cell (A. Belzner, U. B. Bischler, S. Crouch-Baker, T. M. Gur, G. Lucier, M. Schreiber and R. A. Huggins, presented at the 175th meeding of the Electrochemical Society, 8 May 1989, Los Angeles) is complicated by the known differences in heat capacity, thermal conductivity, masses of the evolved gases, electrical conductivity and resistivity of Pd rods between the light-water and heavy-water solutions. These complications make interpretation of differential calorimetric studies of the Pd/H and Pd/D system extremely difficult, and these factors must be accounted for in a quantitative and rigorous fashion before any unusual enthalpy production can be documented.

Minimum voltage requirements for electrolysis

We have also investigated the minimum voltage that is required in a regenerative-cell arrangement to maintain a 64 mA cm^{-2} current density through a Pd rod. This voltage would be obtained when the anode was performing the reverse chemical reaction of the cathode; that is, when the anode is 'depolarized' by introduction of $D_2(g)$. In this situation, the anode could exclusively perform the oxidation of $D_2(g) \rightarrow 2D^+(aq) + 2e^-$ (as opposed to the oxidation of D_2O to $O_2(g)$), and this would result in a lower cell voltage at constant cathode current density. At 64 mA cm^{-2} on a 0.22×10-cm rod with a Pt-foil counter electrode of 30-cm^2 area, our cell voltages are close to that reported in previous work[1], but we have been unable to observe cell voltages <3.8 V even in the presence of a vigorous flow of 1 atm of $D_2(g)$. We attribute this to the relatively low solubility of $D_2(g)$ in D_2O and to the high cell resistance (30–50 Ω) in the 0.1M LiOD electrolyte in the present cell configuration. Also, steady-state current–voltage measurements relative to a Pd/D_2 reference electrode indicated a resistance-corrected potential difference of −0.8 V during cathodic charging of the Pd at current densities of 60 mA cm^{-2}, which is in accord with previous determinations of this potential drop[1]. Thus, the minimum potential difference between a charging Pd rod and an anode at the reversible $D_2(g) \rightarrow 2D^+(aq) + 2e^-$ potential must be >0.8 V and is more typically 3–4 V because of contributions from cell resistance in aqueous 0.1M LiOD. Also, our measurements show that the minimum open-circuit potential difference between a charged Pd rod and a Pd α/β reference electrode is < -0.6 V. Based on these measurements, we conclude that the output/input power ratios >100% reported previously based on an assumed 0.5-V cell voltage[1] are strictly hypothetical quantities. We also conclude that this 0.5-V value is inaccessible on both thermodynamic and practical (resistive loss) grounds, and recommend that any excess power generation rates calculated using this hypothetical voltage be interpreted with these caveats in mind.

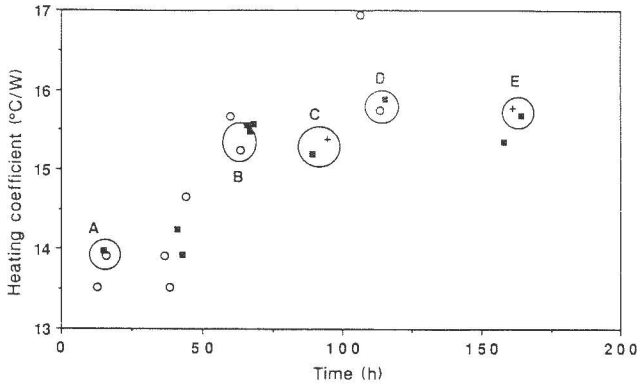

FIG. 3 Heating coefficient plotted against time determined using an isoperibolic calorimetry cell with a Pd/D cathode and a Pt anode in 0.1M $LiOD/D_2O$. The cathode was a cast Pd rod, 0.21-cm diameter × 2.1-cm length. The letters refer to the data pairs given in Table 3. Symbols refer to the following current densities through the Pd/Pt circuit: ○ = 72–74 mA cm^{-2}; ■ = 108–115 mA cm^{-2}; + = 140 mA cm^{-2}.

Conclusions

Within strict error limits, a variety of Pd samples have displayed no anomalous behaviour in the production of excess enthalpy, neutrons, γ-rays, tritium or helium. We have also identified several possible sources of error in isoperibolic calorimetry experiments that might account for excess power production reported in other work. Of course, we cannot exhaust all of the possible variations in heat treatment, impurity content of Pd and D_2O, and so on that might potentially differentiate our procedures from those performed in other laboratories. We note that the present work is not directly relevant to the studies of Jones *et al.*[20] because of differences in the composition of the electrodes and electrolyte. At this point however, evidence supporting a high rate of fusion reactions in Pd that is electrochemically charged with D_2, especially without concomitant radiation production, will clearly require a complete documentation of the methods, the state of the Pd and the error limits in the experiments, as well as a complete explanation of the differences in method between this work and other studies. □

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ACKNOWLEDGEMENTS. Preliminary reports of these results were presented in invited papers at the 1 May 1989 meeting of the American Physical Society in Baltimore and at the 8 May 1989 meeting of the Electrochemical Society in Los Angeles. We thank B. Miller and C. R. Martin for supplying some of the Pd samples used in this work. We also thank N. Hoffmann for obtaining a He analysis of the Pd rods, J. Campbell for use of the tritium scintillation counter, W. Johnson and H. Fecht for supplying the r.f. melted Pd, E.-H. Cirlin for use of the X-ray photoelectron spectrometer, J. Beckett for aid in recasting Pd rods and S. Koonin and R. E. Azuma for discussions. R. Petrasso and S. Luckhardt are acknowledged for several discussions and for a preprint of their analysis of the γ-ray data. This work was supported in part by a discretionary grant from Caltech, by the Office of Naval Research, and by the NSF. This work is contribution 7959 from The Division of Chemistry and Chemical Engineering.

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August 16, 1989
(Via FAX to 9 (801) 581-8433)

Professor Martin Fleischmann
Professor Stanley Pons
Department of Chemistry
2416 Henry Eyring Bldg.
University of Utah
Salt Lake City, UT 84112

Dear Professor Fleischmann or Professor Pons:

As a member of the Subcommittee of the ERAB Cold Fusion Panel charged with evaluating work done on particle emission in cold fusion experiments, I need some specific information that only you can provide. Referring to your original publication in Journal of Electroanalytical Chemistry, I see the gamma ray peak at 2.2 MeV. This has, of course, been reprinted in Nature by Petrasso and co-workers.

But in your Nature article of 06/29/89, you show your signal peak at "2.496 MeV."

In our understanding of results from laboratories all over the world, it is important to us to be aware of problems and sources of error that might affect these results. Therefore I would be most grateful to receive by September 1 a full explanation of this discrepancy.

Thank you very much.

Sincerely yours,

Richard L. Garwin
Forwarded in his absence

cc:
W.L. Woodard, DOE (Via FAX to 9 (202) 586-3119)

RLG:jah:228%MF:081689..MF

Also Adjunct Professor of Physics at Columbia University
(Views not necessarily those of IBM or Columbia)

Energy Research Advisory Board
to the
United States Department of Energy
1000 Independence Avenue, S.W.
Washington, D.C. 20585
(202) 586-5444

August 9, 1989

TO: Cold Fusion Panel

The enclosed materials are circulated for your information.

1. Washington Post Article, 8/8/89.
2. Response from R. Petrasso, 7/31/89.
3. Bigeleisen Letters to Jenson and Huizenga, 8/7/89.
4. Response from Ames Laboratory, 8/3/89.

Bill
William Woodard

Enclosures

080989.WLW

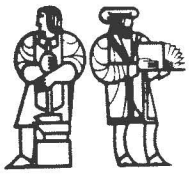
Cold Fusion Funded

■ SALT LAKE CITY—The University of Utah's cold-fusion program cleared a major hurdle when a state panel voted to release $4.5 million for research into the phenomenon and its potential as a new energy source.

The 7 to 1 vote, with one abstention, allows the university to fund a National Cold Fusion Institute for research into fusion and its practical application.

Wilford Hansen, a Utah State University physicist, abstained, and the lone dissenter was university Provost Karen Morse, a chemist who said she would have preferred that more of the money be spent on science instead of facilities and administration.

Washington Post 8/8/89



Plasma Fusion Center
Massachusetts Institute of Technology
Cambridge, Massachusetts 02139
Telephone: 617/253-8100

July 31, 1989

Professor John R. Huizenga
Co-Chairman
Cold Fusion Panel
Energy Research Advisory Board
to the
United States Department of Energy
1000 Independence Avenue, S.W.
Washington, D. C. 20585

Dear Professor Huizenga:

Per the request in your letter of 20 July, please find attached a brief report of our neutron and γ-ray research that relates to cold fusion. Our main findings have been published in two Nature papers (attached), the latter of which I sent you earlier this month (i.e., the exchange between Fleischmann et al. and ourselves). Our report is based on these two papers. To summarize, based on Fleischmann et al.'s own γ-ray spectra, we conclude that they detected neither neutrons nor γ rays from their heat-producing cell.

In addition to the main issues of the ERAB Panel, I hope the Panel will also address the basis by which Fleischmann et al. originally placed their specious neutron-capture-on-hydrogen γ-ray line precisely at 2.22 MeV. Discussion of this was touched upon by Nature's John Maddox in his editorial of 6 July 1989 (340, p. 15, enclosed). Of the Panel members, Steve Koonin and especially Dick Garwin are very cognizant of this issue. If I can be of assistance to the Panel, please contact me.

Sincerely,

Richard Petrasso

Richard D. Petrasso
Physicist
Tel.: 617+253-8458

Encs.

cc: Professor Norman F. Ramsey
Co-Chairman

Absence of Neutrons and γ-Rays from the Fleischmann et al. Experiments*

R. D. Petrasso, X. Chen, K. W. Wenzel, R. R. Parker, C. K. Li, and C. Fiore

Plasma Fusion Center
Massachusetts Institute of Technology
Cambridge, Massachusetts 02139

Our principal findings have been published in two papers in Nature[1,2], summarized here.

Fleischmann et al.[3] originally claimed as compelling evidence of fusion the detection of 2.22 MeV neutron-capture-on-hydrogen γ rays from the water bath surrounding their heat-producing cell. In our first paper[1] we pointed out two fundamental errors in their purported 2.22 MeV γ line: first, the line width is a factor of two narrower than their instrumental resolution would allow; second, a Compton edge should be distinctly evident at 1.99 MeV, but is not present. We therefore concluded that their γ-ray signal is an instrumental artifact and, furthermore, based on γ-ray background radiation, argued the artifact energy was unlikely to be at 2.22 MeV as they had claimed. We suggested the energy position could be easily verified by publication of their full γ spectrum because prominent, naturally occurring background lines from ^{40}K (1.46 MeV) and $^{208}T\ell$ (2.61 MeV) calibrate their spectra absolutely.

In their response to our paper, Fleischmann et al.[4] showed full γ spectra in which they identified their γ-ray signal line to have an energy of not 2.22 MeV but instead 2.496 MeV. They give no explanation for their earlier identification of 2.22 MeV. However, our analysis of their new spectra concludes that their 2.496 MeV "line" as well as the two other adjacent "lines" in the high-energy channels of their spectrum analyzer are instrumental artifacts[2]. We further point out that they have misidentified the $T\ell^{208}$ (2.61 MeV) peak and therefore the actual energy of their purported γ signal line is 2.8 MeV not 2.496 MeV[2]. Of significance is the fact that, using the correct energy calibration for their γ-ray spectra, there is not the slightest hint of any 2.22 MeV neutron-capture γ rays from the water bath surrounding their heat producing cell (relative to their γ-ray background measurements). From our controlled neutron experiment[1,5], we can impose an upper limit on their neutron production rate of 4×10^2 neutrons per second[2]. This bound is 100 times smaller than the rate Fleischmann et al. claimed to have actually observed[3]. In conclusion, Fleischmann et al. observed neither neutrons nor γ rays from their heat-producing cell.

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*Prepared for Energy Research Advisory Board Panel on Cold Fusion, July 31, 1989.

End of cold fusion in sight

Although the evidence now accumulating does not prove that that original observations of cold fusion were mistaken, there seems no doubt that cold fusion will never be a commercial source of energy.

It seems the time has come to dismiss cold fusion as an illusion of the past four months or so. At the outset, on 23 February, the suggestion that deuterium nuclei can be made to fuse together at ordinary temperatures, if in exceptional circumstances, seemed a brave leap of the imagination. The article on page 29 of this issue by M. Gai *et al.* of Yale University is merely another nail in the coffin of the idea. The Yale group has done its best to replicate the conditions of the original experiments, but has failed to replicate their results. Similar outcomes have been reported from other laboratories. So what has been learned from these hectic months?

First, the negative results now being reported do not imply that the original observations by Stanley Pons and Martin Fleischmann at the University of Utah and by Stephen E. Jones and his colleagues at the Brigham Young University were grossly mistaken. Events may yet show that there are circumstances in which palladium electrodes in electrolytic cells emit pulses of neutrons just as they would if deuterium nuclei were fusing together; early this month, a group at the Los Alamos National Laboratory was wondering what to make of such an observation.

So far, all that is clear is that the original reports do not conceal a recipe for making large (Utah) or even modest (Brigham Young) amounts of power by deuterium fusion. For the many non-scientists who have been excited by the past few weeks, this will be a disappointment. By the same test, the managers of orthodox experiments intended to replicate what happens within the Sun will be relieved.

One striking feature of these events is that, even now, those who have been trying to replicate the original findings are remarkably good-humoured about the time and energy they have spent. Those concerned seem to have found it an inherently interesting exercise. It is not, after all, every day that they find themselves worrying about electrochemistry and nuclear physics at the same time. And it is interesting to have been reminded at first hand of the remarkable capacity of palladium and titanium to dissolve hydrogen, usually known only from books and journals. Moreover, the brief spell in April when it seemed as if cold fusion would permanently divide chemists and physicists has left no trace.

All of us, even bystanders, have also learned a great deal about the difficulty of counting absolute numbers of neutrons and of γ-rays. The argument between Richard Petrasso and his colleagues at MIT on the one hand and Pons and Fleishmann on the other about the γ-ray measurements have been for many people educative, to say the least. Petrasso and his colleagues (*Nature* **339**, 183; 1989) first complained of inconsistencies in the only published report by Pons and Fleischmann of their observations, were given an incomplete reply (*Nature* **339**, 667; 1989), but on that basis were able to argue (*Nature* **339**, 667; 1989) that the energy channels in the original equipment had probably been miscalibrated and that the energy spectrum is narrower than the resolution of the γ-ray detector would allow.

There is even doubt about the placing of the γ line purportedly resulting from neutron emission which has been variously reported as at 2.2 MeV and at 2.5 MeV. Pons and Fleischmann originally put it at 2.2 MeV, which is what would be expected if the γ-rays come from the conversion of neutrons in water. But now, in their reply to Petrasso, they say they could not have measured such a peak at such an energy, but that it is in any case at 2.5 MeV (which Petrasso disputes on calibration grounds). The best resolution of this dispute would be by independent measurement, but that seems unlikely while attempts to replicate the phenomenon as a whole are unsuccessful. Meanwhile, there will be many who consider the γ-ray signal to have been an artefact.

That point is nevertheless crucial to the unfolding of events after 23 February, when both *The Wall Street Journal* and *The Financial Times* published long accounts of what had been done at Utah and when the University of Utah held a press conference to tell the wider world. (*Nature* owes Pons and Fleischmann an apology for having reported that, on that occasion, they had said that their formal paper had been sent to this journal for publication.)

It is unthinkable that reports of the production of excess heat in such complicated electrochemical cells would, by itself, have been seriously regarded as proof of deuterium fusion. Only the measurement of nuclear particles and products, with the expected energy, could have commanded the interest since shown. Pons and Fleischmann now say that "as we have repeatedly pointed out, we are well aware of the deficiencies of these spectra", but there are no records of that reservation earlier than the meeting of the US Electrochemical Society at Los Angeles, by which time they had be[illegible]nt (but may not have read) Petrasso's [illegible]rst draft of his complaint. This is a more serious retreat than they acknowledge.

None of this implies that Pons and Fleischmann have been anything but straightforward. Put yourself in their position if you believe otherwise. If, on 23 February, you had made such an arresting announcement that the whole world was agog, even picking up the telephone would probably engage you in a half-hour conversation with somebody you had never met. Your compelling interest, to gather more data, would be compromised by the inquisitiveness of people asking elementary questions about issues then, in your mind, settled. It is remarkable that Pons and Fleischmann, with all the pressure on them, should have been able to cover so much ground.

So how should they, in the contemporary argot, have played it? It is too easy to say that they should never have given the story of their doings to the financial newspapers, or have allowed a press conference to be held on their behalf. The conventional wisdom, that they should have sent an account of their work to a respectable science journal and then have put themselves in the hands of its referees and editors, is too bland. If people believe they have found a way of changing the world, why should they not tell the world what is in store in their own way?

But there are obvious dangers in such a course, of which the chief is that one may be mistaken. Ordinarily, there is no shame in that: people make mistakes all the time. Ordinarily, there are also colleagues to point to pitfalls in one's path, but potential sceptics may on this occasion have been denied access by the care with which the project was kept secret over five years. It is less easy to accept that one may afterwards be required to accept irksome conditions on how one practises research by an over-confident university; from about 24 February, Pons and Fleischmann might well have decided that they should put their responsibility to the scientific community before that to the organizer of their press conference. Even now, it would be interesting if they made their data generally available, whatever its correct interpretation.

John Maddox

Measurement of γ-rays from cold fusion

SIR—Petrasso *et al.*[1] have recently published a critique of the γ-ray spectrum given by three of us[2] as supporting evidence for the solid-state fusion of deuterons in palladium host lattices. The basis of this critique was the nature of a γ-ray spectrum displayed during a television broadcast. One of us (M.H.) denies the accuracy of ref. 6 of Petrasso *et al.*; M.H. did not state that the quoted television spectrum was made in these laboratories, as it most certainly was not. In view of this somewhat strange approach to the collection of scientific data and, as we cannot vouch for the authenticity of the spectrum transmitted (we have now confirmed that the "curious structure" in the television 'data' given by Petrasso *et al.* — their Fig. 1*b* and legend[1] — is simply the trace of a screen cursor on the multi-channel analyser visual display unit!), we give in the first place one of the complete set of spectra recorded at that time (Fig. 1).

In the work reported by us[2], γ-ray spectra were measured principally to check on the safety of our operations and, as we have repeatedly pointed out, we are well aware of the deficiencies of these spectra. Figure 1 gives the background spectrum ('sink'; solid line) taken over a sink containing identical shielding materials but at a distance of 5 m from the tank containing the experimental cell. This cell contained a 0.4 × 10 cm palladium electrode polarized at a current density of 64 mA cm^{-2}; during the period of the measurement it was generating excess heat at the rate of 1.7–1.8 W (over and above that due to the electrode reactions).

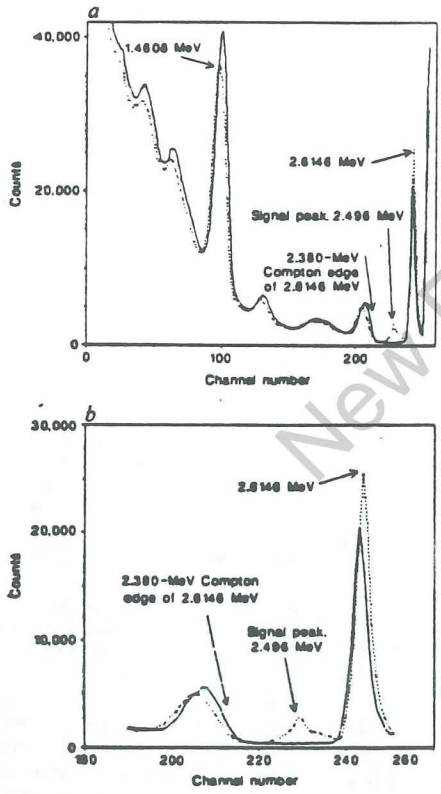

FIG. 1 The γ-ray spectrum accumulated over the water bath containing the electrolytic cells ('tank'; dotted line) and over a sink 5 m away ('sink'; solid line). Detector is a 3 × 3 in. NaI right circular cylinder. Spectral accumulation times: 50 h. *b* is an expanded version of the most relevant region of *a*.

The 'peak' under discussion is centred at 2.496 MeV, and it can be seen in Fig. 1*a* that all the peaks in the background spectrum on the low-energy side of 2.496 MeV are displaced to higher energies whereas that on the high-energy side (due to ^{208}Tl) is displaced to a lower energy. (Scaling of the spectrum made near the electrolytic cell with a quadratic interpolation formula generated the spectrum we reported: this scaling produced a shift and narrowing of the 2.496-MeV peak.) The observed shifts are due to a combination of zero shift in the analyser and a gain shift of the NaI detector resulting from drift of the pre-amplifier. Over the long data-acquisition times, the shifts are of little importance. The spectra do indicate, however, that the nature of the background radiation in the two areas of the laboratory is essentially the same. The only significant difference between the spectra is the signal peak. This is very convincing evidence that the signal peak is not due to products of radon decay.

It can be seen, however, that there is another unexplained feature in these spectra: there is a rising tail at the end of the spectra. This is due to pulse pile-up in the last few channels as a result of a peak at slightly higher energy than the 2.6146-MeV peak (Fig. 2). Figure 2 represents a background spectrum that was acquired with a slightly reduced gain so that the energy window could be extended.

The exact interpretation of the 2.496-MeV peak is in doubt; certainly, the peak from the reaction $^1H + n \rightarrow {}^2D + \gamma$ (2.22 MeV) would be expected to lie to the left of the Compton peak that arises from the thorium decay chain. The search for this peak does not seem to be feasible using NaI detectors. In spite of the problems underlying the interpretation of these spectra, we consider that the measurements show the emission of γ-rays from the cell environment: removal of the cells leads to the removal of the signal peak. A possible interpretation is that the signal peak is a single- or double-escape peak from 3.01- or 3.52-MeV peaks, or from summing of other unidentified peaks at lower energies. The unusual shape of the signal peak suggests that it may be a combination of such peaks. The size and energy of the signal peak imply that any associated Compton edge or escape peak will be lost beneath the rest of the spectrum.

Petrasso *et al.*[1] have also commented on

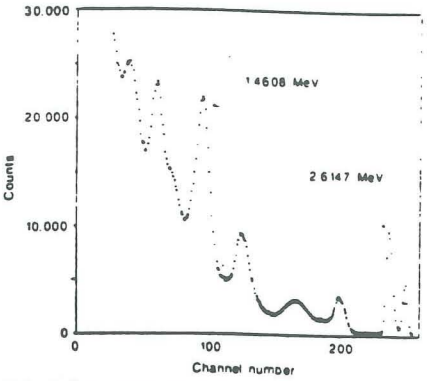

FIG. 2 The γ-ray spectrum accumulated in a similar manner to those in Fig. 1 in a remote laboratory at reduced gain.

the integrated peak intensity of the γ-ray spectrum reported by us and imply that we sought to relate this to the neutron count observed close to a similar cell operated in the open air. We point out that we made no such comparison but instead sought to relate the neutron count rate to the tritium production which we and others have observed. Clearly, further work on the γ-ray spectra should include the characterization at high resolution with solid-state intrinsic germanium detectors of the γ-ray emissions in the energy region above 2 MeV.

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PETRASSO *ET AL.* REPLY—Our criticism[1] of the published 2.22-MeV neutron-capture-on-hydrogen γ-ray line of Fleischmann *et al.*[2], claimed by them as compelling evidence of neutron production in their electrochemical cells (Fig. 1*a* of erratum of ref. 2; Fig. 2 of ref. 1; our Fig. 1 here), raised two fundamental points: Fleischmann *et al.*'s γ-ray line first is a factor of two narrower than their instrumental resolution would allow, and, second, lacks a Compton edge, which should be distinctly evident at 1.99 MeV (Fig. 1). We therefore concluded that their γ-ray signal line was an instrumental artefact, and we argued that the energy position of their signal line was unlikely to be at 2.22 MeV as they claimed. We suggested that the energy of the signal line could easily be verified by publication of their full γ-ray spectrum, because prominent, naturally occurring background lines from ^{40}K (1.46 MeV) and ^{208}Tl (2.61 MeV) calibrate their spectra absolutely[1,3,4].

In their response above, Fleischmann

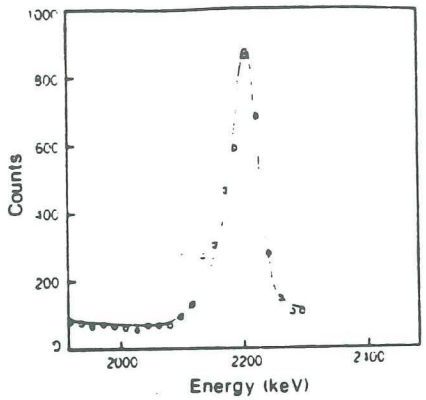

FIG. 1 A reproduction of the purported 2.22-MeV neutron-capture-on-hydrogen γ-ray line of Fleischmann *et al.* (Fig. 1*a* of ref. 2, erratum). As we discuss in ref. 1, the resolution of their NaI spectrometer would be about 2.5% based on this linewidth. With such resolution, one would expect to see a clearly defined Compton edge at 1.99 MeV. No edge is evident. Also, a resolution of 2.5% is inconsistent with their spectral resolution (Table 1*b* of ref. 1). Because of these inconsistencies, we argue that this signal is an instrumental artefact[1]. In this figure, Fleischmann *et al.* show the peak energy to be at 2.22 MeV. In their response above, they indicate that their γ-ray signal line has an energy of 2.496 MeV (see peak 7 of Fig. 2*a* and text).

et al. fail to address our key criticisms concerning their published 2.22-MeV γ-ray line. They further claim, erroneously, that their televised γ-ray spectrum[1] was the basis of our analysis. (Our quantitative analysis is based on their published signal line (Fig. 1), their instrumental resolution (Table 1*b* of ref. 1), a controlled neutron experiment (Fig. 3 of ref. 1) and the well-known properties of NaI scintillation detectors[5,6].) Fleischmann *et al.* do, however, show their full γ-ray spectra (our Fig. 2*a*), in which they identify a signal line to have an energy not of 2.22 MeV, but now of 2.496 MeV. (No explanation is given for their original identification of 2.22 MeV (Fig. 1).) They contend nonetheless that this signal line is proof of true γ-ray emissions from their heat-producing cell, although unrelated to the 2.22-MeV neutron-capture γ-ray. Unfortunately they are unable to identify the nuclear process that generates this 2.496-MeV γ-ray, or to account for its distinctly unphysical lineshape. We again suggest that their signal line is an instrumental artefact unrelated to a γ-ray interaction — this holds both for their signal line in the errata of ref. 2 and for that in their response above (peak 7 of our Fig. 2*a*).

In regard to their γ-ray spectra (Fig. 2*a*), we argue that Fleischmann *et al.* have misidentified the ^{208}Tl peak and therefore that their energy calibration and interpretations are correspondingly suspect. We have numbered the peaks in their spectra so as to best match our energy identifications[1,3,4], and we show beneath their γ-ray spectra one obtained at MIT with a 3 × 3 in. NaI(Tl) crystal (Fig. 2*b*)[1,7]. (See refs 1 and 2 for a discussion of the 3 × 3 in. NaI detector used by Fleischmann *et al.*[2].) The energy scales in Fig. 2 have been scaled and aligned so that the ^{40}K and (true) ^{208}Tl peaks (peaks 3 and 6, respectively) coincide for both spectra. There is a clear correspondence between the Fleischmann *et al.* and MIT spectra for all energies below the (true) ^{208}Tl peak at 2.61 MeV. To further test our energy identifications for the Fleischmann *et al.* spectrum, we plot in Fig. 3 their channel number against our line-energy identifications. The relationship is approximately linear, as it should be if our identifications are sensible. (For the weaker 'peaks' (1, 2, 4 and 5) the precise centroid of energy is a function of the local terrestrial radiation environment[4].) Based on this calibration curve (Fig. 3), their purported γ-ray signal line resides at about 2.8 MeV, not at 2.496 MeV.

The next issue is to identify the other features in the spectra of Fleischmann *et al.* (peaks 7, 8 and 9 in Fig. 2*a*) above the (true) ^{208}Tl line (peak 6). Peak 8, very prominent in both background (sink; solid line) and the cell (tank; dashed line) experiments, and identified by Fleischmann *et al.* as the ^{208}Tl peak, is unphysical, as its linewidth is more than two times smaller than their instrumental resolution would allow (based on the ^{40}K linewidth in Fig. 2*a*; see also refs 1, 6, 7). This is exactly the same objection that we raised in regard to their purported 2.22-MeV γ-ray line (Fig. 1). This observation especially reinforces our contention that peak 6, rather than 8, is to be identified with the ^{208}Tl line. A similar criticism of the linewidth can be made of peak 9 (see Fig. 2 of Fleischmann *et al.* above). The unphysical shape of peak 7, their signal line, also argues against its identification as a real γ-ray line. Thus we conclude that all 'γ-ray signals' above the (true) ^{208}Tl peak (6) are instrumental artefacts. We further point out that in our background spectra (Fig. 2*b*) and in the standard works on γ-ray background radiation[3,4], there is no evidence for any strong lines above the (true) ^{208}Tl peak. Therefore, Fleischmann *et al.*'s present claim of having observed γ-ray emissions from their cell is unfounded.

Another important issue is whether 2.22-MeV neutron-capture γ-rays actually emanate from the water bath surrounding their heat-producing cell which, according to ~~ref. 2,~~* generates 1.7–1.8 W. Using our energy calibration for their spectral data (Fig. 3), and looking in the vicinity of peak 5 of Fig. 2*a* (which is close to 2.22 MeV), we find no evidence for even a small change in the background spectrum relative to that of their heat-producing cell. Quantitatively, from our controlled neutron experiment in a water bath[1,7] and the spectral data of Fig. 2*a*, we can estimate an upper limit on the neutron production rate of about 4 × 10^2 neutrons s^{-1}. This bound is a factor of 100 smaller than the rate Fleischmann *et al.* claim to have actually observed with their neutron detector[2]. (Fleischmann *et al.* purport above that NaI scintillation detection is inadequate to detect 2.22-MeV neutron-capture γ-rays. This view is erroneous[1,5,6].)

In conclusion, Fleischmann *et al.* fail to answer our key criticisms[1] of their published γ-ray spectrum[2], in particular the spurious nature of their purported 2.22-MeV neutron-capture γ-ray line (our Fig. 1). Although they inexplicably no longer claim to have observed the 2.22-MeV neutron-capture γ-ray, they now

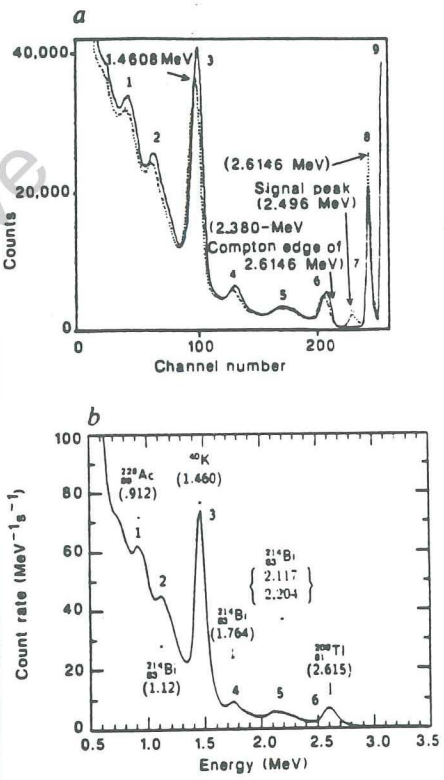

FIG. 2 The γ-ray spectra from *a*, the Fleischmann *et al.* experiments (solid and dotted lines are for 'sink' and 'tank', respectively), and *b*, the MIT γ-ray background measurements. In these two figures, the various peaks have been identified with numbers. (Peaks 1, 2, 4 and 5 are actually blendings of many lines[3,4] of which we have identified the main contributors.) In *a*, Fleischmann *et al.* have identified the ^{208}Tl line with peak 8; we believe that it should instead be peak 6. Consequently we have placed in parentheses their energy identifications and remarks which we believe are in error, and we have accordingly recalibrated their data. With this recalibration, *a* and *b* have been drawn on the same energy scale. Fleischmann *et al.* identify peak 7 as their γ-ray signal line, that is, as the indicator of actual nuclear γ-rays emanating from their heat-producing cell. They now identify its energy as 2.496 MeV, not 2.22 MeV as in their earlier work[2]. (See also Fig. 1.) Using our energy calibration (Fig. 3), peak 7 corresponds to an energy of about 2.8 MeV. We argue that the Fleischmann *et al* peaks of 7, 8 and 9 are instrumental artefacts; see text.

* their report above,

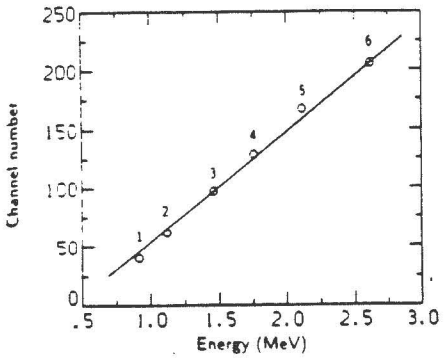

FIG. 3 The energy calibration for the γ-ray spectrum of Fleischmann *et al.* (tank spectrum, dotted line of Fig. 2*a*) using the energy identifications of Fig. 2*b*. Note that we have identified the ^{208}Tl line (2.61 MeV) with peak 6, not 8 as Fleischmann *et al.* did. The straight line is drawn through the ^{40}K (1.46 MeV) and the ^{208}Tl (2.61 MeV) points, which are the most prominent background lines[3,4].

contend that their γ-ray signal line is a true γ-ray of energy 2.496 MeV and, most importantly, that this signal is evidence for nuclear reactions in their cell. They make this claim despite their inability to identify the nuclear process associated with their purported 2.496-MeV γ-ray or to account for its distinctly unphysical lineshape. Furthermore, after correcting their spectral data for a miscalibration in energy, we argue that their signal line (with a correct energy of 2.8 MeV rather than 2.496 MeV) as well as all lines above their (true) ^{208}Tl peak (2.61 MeV; peak 6) are instrumental artefacts in the upper channels of their spectrum analyser. Finally, using our calibration for their γ-ray spectra (Fig. 3), we find no evidence for the emission of 2.22-MeV neutron-capture-on-hydrogen γ-rays from the water bath surrounding their heat-producing cell. Quantitatively this can be interpreted as imposing an upper bound of 4×10^2 neutrons s^{-1} for the production of neutrons. This limit is a factor of 100 times smaller than the neutron rate Fleischmann *et al.* claim to have actually observed[2].

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Problems with the γ-ray spectrum in the Fleischmann *et al.* experiments

SIR—Fleischmann, Pons and Hawkins[1] recently announced the observation of significant heating in their cold-fusion experiments, a result that they attribute to copious fusion reactions. As compelling evidence that fusion had occurred, they reported the observation of the 2.22-MeV γ-ray line that originates from neutron capture by hydrogen nuclei[2,3]

$$n + p \rightarrow d + \gamma\,(2.22\ \text{MeV}) \qquad (1)$$

(Here d represents a deuteron.) They contend that the neutron in reaction (1) is generated by the reac

$$d + d \rightarrow n + {}^{3}He \qquad (2)$$

and conclude, therefore, first that the 2.22-MeV γ-ray confirms that the fusion process (2) is occurring, and second that a neutron production rate of the order of 4×10^{4} neutrons s^{-1} is derivable from their γ-ray signal rate. They further state that most of the heat generation occurs not through process (2), but through a hitherto unknown nuclear-fusion process.

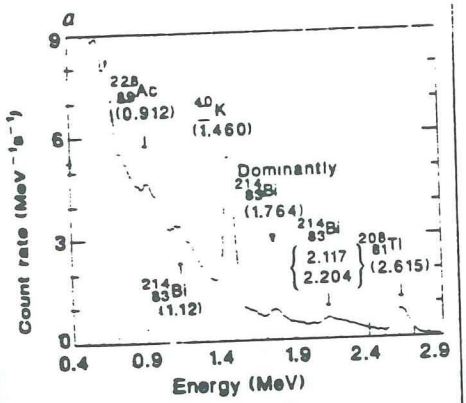
Here we focus solely on the identity of the reported γ-ray line, which we shall henceforth call the signal line. We argue that the claim of Fleischmann *et al.* to have observed the 2.22-MeV line characteristic of reaction (1) is unfounded. We do so on the basis of three quantitative considerations: (1) that the linewidth is a factor of two smaller than their instrumental resolution would allow; (2) that a clearly defined Compton edge[4], which should be evident in their published data at 1.99 MeV, is not in fact present; and (3) that their estimated neutron production rate is too large by a factor of 50. In addition, from a consideration of the terrestrial γ-ray background, we argue that their purported γ-ray line actually resides at 2.5 MeV rather than 2.22 MeV. These conclusions are, in part, based on our studies of neutron capture by hydrogen, using a neutron source submerged in water. These measurements allow us to compare the results of Fleischmann *et al.* directly with a controlled experiment.

We measured terrestrial γ-ray background spectra in order to compare our detector characteristics with those of Fleischmann *et al.* Figure 1*a* shows a typical terrestrial γ-ray background spectrum obtained with a 3 in. × 3 in. NaI(Tl) crystal spectrometer system (see ref. 5 for details). The main features of the background spectrum are quite similar throughout the terrestrial environment[6,7]. Fleischmann *et al.* showed a similar γ-ray spectrum on television (Fig. 1*b*). (We believe that we have viewed all the cold-fusion γ-ray spectra that have been shown on KSL-TV (Utah) up to 19 April. This information was obtained from Utah News Clips, Inc., Utah. As far as we can tell, all spectra are identical to that of Fig. 1*b*.) This spectrum was obtained in the course of the Fleischmann *et al.* experiments (M. Hawkins, personal communication). Their spectrometer system consisted of a Nuclear Data ND-6 portable analyser with a 3 in. × 3 in. NaI(Tl) crystal (ref. 1 and M. Hawkins and R. Hoffmann, personal communications). A ⅜-in.-thick Pb annulus encompassed the scintillator. It is clear from Fig. 1*a* and *b*, and particularly from the ^{40}K (1.46 MeV) and ^{208}Tl (2.61 MeV) lines, that our resolution is comparable to or better than that of the spectrometer used by Fleischmann *et al.*, a point to which we return later.

In the interval 1.46–2.61 MeV, the energy resolution of a NaI(Tl) spectrometer, which determines the γ-ray linewidth, can be well described by the formula[8,9]

$$R(E) = \frac{\Delta E}{E} \approx R(E_n)\sqrt{E_n/E} \qquad (3)$$

Here ΔE is the full width at half maximum (FWHM) of the line, E is the energy of the photon and $R(E_n)$ is the measured 'reference' resolution at energy E_n. $R(E_n)$ can be accurately determined using a ^{60}Co source (that is, the ^{60}Co line at 1.33 MeV), or it can be fairly well approximated by the ^{40}K decay line at 1.46 MeV. (From Fig. 1*b*, the ^{40}K decay line allows one to estimate Fleischmann *et al.*'s resolution as ~ 8%.)



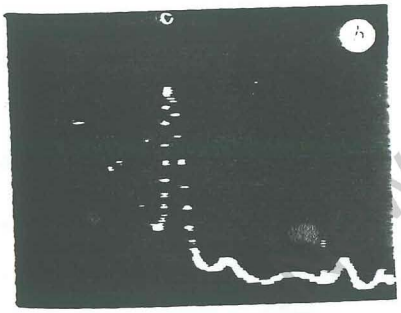


FIG. 1 *a*, The γ-ray background spectrum measured with a 3 in. × 3 in. NaI(Tl) detector at MIT. Some important terrestrial γ-ray lines have been identified in this figure[6,7,12]. (As explained in ref. 12, the immediate parent of the final decay product is identified. For example, ^{40}K β^{+} decays into an excited nuclear state of ^{40}Ar, which actually then emits the 1.460-MeV photon discussed in the text.) The spectrum is averaged over an 84-hour run. *b*, The γ-ray spectrum shown on television by Fleischmann *et al.* The main characteristics of the two spectra are similar; one can also tell that the two detectors have comparable spectral resolution. In *b*, note the curious structure at about 2.5 MeV and that beyond the ^{208}Tl peak (2.61 MeV), which appear to be artefacts. (The spectrum can also be obtained from KSL-TV in Utah (M. Hawkins, personal communication).)

TABLE 1 Comparison of energy resolutions of the γ-ray spectrometers

(a) Resolution of MIT spectrometers

Energy (MeV) Origin	1.17 ^{60}Co	1.33 ^{60}Co	1.46 ^{40}K	2.22 n(p,γ)d	2.61 ^{208}Tl
Natural background			0.055		0.043 (0.041)
^{60}Co	0.056	0.051			
Pu/Be neutron source				0.05 (0.045)	

(b) Resolution of the Fleischmann et al. spectrometer[1]

Energy (MeV) Reference	1.33 ^{60}Co	1.46 ^{40}K	2.22 n(p,γ)d	2.61 ^{208}Tl
Hoffman*	0.056	0.065		~0.05
TV news†		~0.08		(0.049)
Ref. 1 (errata)			0.025 (0.053)	

The resolution is defined as the full width at half maximum (FWHM) divided by the peak energy. Numbers in parentheses are predicted values based on the detector resolution at 1.46 MeV (see text). In *b*, the prediction is based on the resolution value (0.065 at 1.46 MeV) provided by R. Hoffman (personal communication).
*R. Hoffman (personal communication).
†Derived from images of the televised news broadcasts.

Table 1 lists the resolution data for our detectors and for that of Fleischmann *et al.*.

We now compare the signal line of Fleischmann *et al.* (Fig. 1*a* of ref. 1 (errata), shown as Fig. 2 here) with our measured spectrum obtained from the experiments on neutron capture by hydrogen (Fig. 3 here, and Fig. 4 of ref. 5). In these experiments, a Pu/Be neutron source was placed in a water tank. $^{239}_{94}$Pu emits energetic α-particles, which produce neutrons through (α, n) reactions with Be (refs 4,9). The neutrons are thermalized in water, and we observe the emitted fission-neutron-capture γ-rays with our spectrometers. The measured resolution at 2.22 MeV is ~ 5% (Table 1*a*), and is reasonably well predicted by equation (3). As a consequence, this calls into immediate question the identity of Fleischmann *et al.*'s signal line as a γ-ray line. Specifically, Fig. 2 shows the signal line to have a resolution of 2.5%. This is about a factor of two smaller than that predicted by equation (3) on the basis of the known resolution (Table 1*b*) from either the ^{40}K decay line (1.46 MeV) or from the ^{60}Co source (1.33 MeV) (R. Hoffman, personal communication). But we know from Table 1 that the spectrometer used by Fleischmann *et al.* has a resolution that is at best comparable to our own for the entire region from 1.46 to 2.61 MeV (see also Fig. 1), so it is inconsistent that their linewidth at 2.22 MeV is a factor of two below the predicted value.

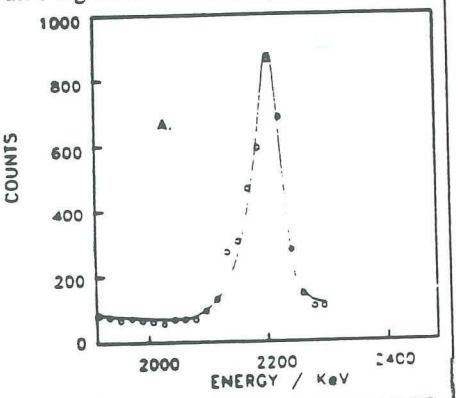


FIG. 2 A reproduction of the purported 2.22-MeV γ-ray signal line of Fleischmann *et al.* (Fig. 1*a* of errata to ref. 1). The resolution, based on the linewidth, is about 2.5%. With such resolution, one would expect to see a clearly defined Compton edge at 1.99 MeV. No edge is evident. Also, a resolution of 2.5% is inconsistent with their spectral resolution. Furthermore, we argue that the signal line may reside at 2.5 MeV, not at 2.22 MeV as is claimed by Fleischmann *et al.* and depicted here.

There is a second crucial inconsistency with the published signal line (Fig. 2). If we assume a resolution of 2.5% at 2.22 MeV, then there should be a clearly defined Compton edge[4] at 1.99 MeV. For example, in Fig. 3 the Compton edge is evident even for our measured resolution of only 5%. For a resolution of 2.5%, the definition of the Compton edge would be distinctly sharper. The lack of a Compton edge at 1.99 MeV for the signal line therefore negates the conclusion of Fleischmann *et al.* that they have observed the 2.22-MeV γ-rays from neutron capture by hydrogen.

We also point out that in our (Pu/Be) neutron-capture experiments, a conspicuous $e^+ - e^-$ annihilation single-escape peak exists at 1.71 MeV (Fig. 3), as well as a double-escape peak at 1.20 MeV. (The full spectrum from the Pu/Be experiment, as well as the background spectrum, can be found in ref. 5.) Such features unambiguously identify the primary γ-rays as having an energy of 2.22 MeV, and are a necessary consequence of the physical processes of detection of γ-rays in a finite-sized NaI scintillator.

Based independently on both their γ-ray and neutron measurements, Fleischmann *et al.* claim to have observed a neutron production rate of ~ 4×10^4 neutrons s^{-1} (ref. 1). This claim is clearly inconsistent with their γ-ray signal line, for the following quantitative reasons. The Pu/Be neutron source used in our experiment is absolutely calibrated to within 10% of 1.5×10^6 neutrons s^{-1} (ref. 10 and MIT Reactor Radiation Protection Office). In obtaining the data in Fig. 3, we used an experimental setup similar to that of Fleischmann *et al.* (ref. 1; televised broadcasts; and M. Hawkins and R. Hoffman, personal communications). Our Pu/Be source was submerged 6 in. into a large water tank. The rate at the 2.22-MeV peak, after subtracting the background continuum, is about 1.4×10^3 MeV^{-1} s^{-1} (see Fig. 3). Scaling this rate to a neutron source of 4×10^4 neutrons s^{-1} (the level given by Fleischmann *et al.*), and integrating over the linewidth, gives a total 2.22-MeV γ-ray rate of about 4.5 counts per second. This value is a factor of 50 times higher than the rate that would be calculated on the basis of the results in Fig. 2 (that is, 0.081 counts per second). (Fleischmann *et al.* state that their neutron count rate is measured with a BF$_3$ neutron counter over a 0.4 mm × 10 cm Pd cell, and that the γ-ray measurement is over a 0.8 mm × 10 cm Pd cell[1]. If the total reaction rate is proportional to the volume of Pd rod, as they state, the inconsistency in the reported neutron rate is by a factor of 200 rather than 50.) While differences in rates of a factor of two might possibly be explained by geometrical considerations, a factor of 50 is inexplicable.

A further point concerning the identification of the signal line is the precise value of the energy at which the peak occurred. From Fig. 2, the background in the neighbourhood of the peak is seen to be ~ 80 counts per channel, a level that corresponds to ~ 400 counts per channel for a 48-hour accumulation time (the data in

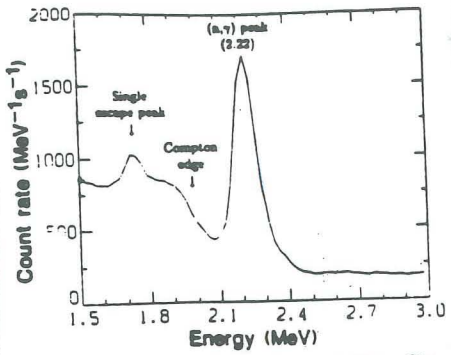


FIG. 3 The γ-ray spectrum measured by a 3in. × 3in. NaI(Tl) spectrometer during a neutron-capture-by-hydrogen experiment using a (Pu/Be) neutron source submerged in water. Because of the finite size of the crystal (which is identical to that of Fleischmann *et al.*[1]), we also see an escape peak[2-4] and, of particular importance here, the Compton edge[4]. In this figure, the digitization energy width is 0.024 MeV per channel. The full Pu/Be and background spectra are shown in Fig. 4 of ref. 5.

Fig. 2 were accumulated for a period of 10 hours[1]). On the other hand, in the Utah measurements of terrestrial γ-ray background, the level in the vicinity of the 2.22-MeV feature was found to be ~ 4,000 counts per channel (R. Hoffman, personal communication). The only relevant part of the entire γ-ray spectrum (between 1.46 and 2.61 MeV) in which the background was as low as 400 counts was at an energy in the vicinity of 2.5 MeV (R. Hoffman, personal communication). Thus, we argue that the peak in the spectrum shown in Fig. 2 may be at 2.5 MeV, not at 2.22 MeV.

The importance of properly identifying the energy of the feature claimed by Fleischmann *et al.* can hardly be overemphasized. Thus, it is extremely unfortunate that they chose to display only the energy range 1.9–2.3 MeV in their pub-

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lished Fig. 1*a*, thereby not providing the supporting evidence of the ^{40}K (1.46-MeV) and ^{208}Tl (2.61-MeV) features which must be present in their spectra in order for their identification to be correct.

Therefore, although Fleischmann *et al.* may have observed a change in their γ-ray spectra that bears some relation to detector location, we conclude that it is unrelated to the 2.22-MeV neutron-capture γ-rays, and that it is also unrelated to the background $^{214}_{83}$Bi line (2.20 MeV; Fig. 1*a*), as has been suggested elsewhere[11]. We can offer no plausible explanation for the feature other than it is possibly an instrumental artefact, with no relation to a γ-ray interaction.

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Fusion In from the ...old?

SIR—Recent experiments involving the loading of deuterium (D) into palladium[1] via a 0.1 M LiOD/D_2O electrolyte may have created something not encountered in the absence of current flow. The voluminous body of older work on electrolytic D–Pd loading is based on thin wires or foils of high aspect ratio which allow only small voltage differentials. But the reported chemical potential across a thick deuterated palladium negative elctrode could give rise to metastable solid phases alien to the existing equilibrium Pd–D phase diagram. The theoretical existence of palladium analogues to hydrogen-rich compounds like Li_2ReH_9 is as tantalizing as the limit of solid solubility of D in Pd is finite. And beyond PdD_3 (by analogy to the known compound Li_3Pd) the thermodynamic distinction between the higher palladium deuterides and metallic deuterium grows dim. The mechanism of their formation might involve the intersection of high deuteron mobility grain boundaries with the Cottrell clouds of H and D that decorate dislocations in many transition metals.

A small mass of (mostly) deuterons transforming into molecular deuterium as they recapture electrons previously delocalized into the S- and D-bands of palladium could release roughly 1 MJ mol^{-1}. Apart from explaining the electrode meltdown and vaporization reports[1] (although this could be due to lithium's remarkable ability to lower the melting point of palladium), this could raise local temperatures to very high values.

Could the so-called 'cold fusion' environment in fact involve local temperatures of greater than 10^5 K generated by the detonation of deuteron clusters (R. G. Gordon, personal communication) or of a metallurgical precipitate, coarse or fine-grained, of a high D/Pd ratio intermetallic compound inside a deuterated palladium electrode? The energy of reassociation of electrons and deuterons is roughly 1 Rydberg (13.6 eV) minus the work-function of Pd (4.9 eV); to this must be added the roughly 4.7 eV liberated when two deuterium atoms pair. Obviously, these energies must be scaled down to compensate for the difference between reactions in free space and the solid state. Nevertheless, the 20 eV energy of formation of D_2 from deuterons could thus produce hot and highly compressed deuterium plasma bubbles of small (>0.01μm–<100μm) size. As to the objection that the surrounding cool metal will quench these bits of pale fire in a nanosecond or so, I believe it answers a serious question: how come the Fleischmann and Pons[1] claimed neutron yield is 9–14 orders of magnitude short of their claimed heat flux? At present, one can only speculate as to by how many orders of magnitude a reflected spherical shock front might raise the temperature and pressure of the D-plasma.

There is another ramification to the notion of nano-novas flashing out of perdeuteride grains decorating the grain boundaries of electrochemically overwrought palladium nagative electrodes. The explosion of such precipitated grains will, above a critical radius, generate cracks in the adjacent metal. It has been postulated (G. Chapline, personal communication) that the surfaces of such cracks, as they open, could host a field gradient or a propagating array of plasmons down whose wake field a deuteron could accelerate. This presumes that the bulk Pd–D system has reached 1:1 stoichiometry and thus been restored to long-range order; this condition is very far from the disorderly and anharmonic state of Pd–D during the early stages of electrolytic D-loading. In this model some thousands of unit cells of travel would suffice to yield keV deuterons. So the propitiated shade of Rutherford may yet countenance not-so-cold fusion.

It has been noted in a News and Views article in the 27 April issue[3] that neutrons have been observed when cracks are generated in crystals of lithium deuteride[4].

RUSSELL SEITZ

Center for International Affairs,
Harvard University, Cambridge,
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Mössbauer cancer therapy doubts

SIR—Mills *et al.*[1] present data to support the view that a dose of 10^{-5} Gy of 14.4-keV X-rays can ablate a population of malignant cells containing ^{57}Fe(III) · bleomycin. They suggest that such a regime may have the potential for the low-dose sterilization of superficial human tumours.

This is unlikely on simple physical grounds, basically because only a small fraction of the exposed cells will have received any energy deposition at all. The proportion of cells that would receive one or more energy depositions, assuming the statistical independence of such events, is obtained from the Poisson distribution and is $(1 - e^{-n})$; here n, the average number of energy depositions occurring in the sensitive site, is given by D/z_{1F}, where D is the absorbed dose and z_{1F} is the so-called 'frequency-averaged specific energy per event' — or simply, the mean energy per unit mass deposited by single energy deposition events in the sensitive site in the cell[2]. This mean specific energy will be similar, irrespective of whether the photon is ultimately absorbed in a photoelectric event or in a Mössbauer absorption (it would be slightly larger in the latter case); it has been measured for 12-keV photons in a tissue-equivalent material in spherical sites of various volumes[3].

Appropriate volumes for consideration are those of typical human cell nuclei (100–1,000 μm^3)[4] or, perhaps more relevantly, the volume of nucleotides in the mammalian nucleus (~3 μm^3)[5]. For volumes of 250 and 3.5 μm^3, for which measurements have been made[3], the corresponding values of z_{1F} are 4×10^{-3} Gy and 0.14 Gy. These numbers yield a probability of about 2.5×10^{-3} that a 250 μm^3 volume of cell nuclei will be subject to at least one energy deposition, and a corresponding probability of 7×10^{-5} for a 3.5 μm^3 volume of nucleotides. Thus, only about 1 in 400 cells (250 μm^3 volume) or 1 in 14,000 cells (3.5 μm^3 volume) would receive any energy deposition at all if exposed to a dose of 10^{-5} Gy.

To sterilize even a small tumour containing about 10^8 cells requires an appropriately small probability (less than 10^{-8}) that any cell will receive no energy-deposition events. Thus, an average number of energy depositions per cell of

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StonyBrook

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7 August 1989

Professor John R. Huizenga,
Department of Chemistry,
University of Rochester,
Rochester, New York 14627

Dear John,

I enclose a copy of a letter (paper mail plus FAX) that I am sending to Reed Jensen, coordinator of the Los Alamos cold fusion effort. I have discussed the content of this letter with Jensen and he has agreed to furnish the information. I am following this up for the following specific reasons. We need to get a statement from Los Alamos about the conditions under which they may have observed electrochemically produced tritium. Reed Jensen told me that they observed tritium at the level of 10^{11} T atoms/cc D_2O (10^4 dpm/ml. D_2O) in 2 cells out of 50. (Maurice Goldhaber, with his characteristic one liners, remarked at a recent picnic that this places an upper limit of 4% that cold fusion is correct.

The request for information about the analysis of the Texas A&M sample is aimed at getting a handle at the Texas A&M moving target. Bockris now claims that the T is formed as DT in solution and then comes out into the gas phase. After I suggested that he look for T in the gas phase, they claim to find it. Since DT exchanges very, very slowly with $D_2O(l)$ in the absence of a catalyst (the only one known to date is the Chalk River catalyst), there should be no T in the D_2O after the liquid is no longer in contact with the D_2 gas.

Could you authorize Bill Woodard to send a follow up questionnaire requesting information on items 1 - 8 and also the paragraph immediately following item 8 in my letter to Reed Jensen. The questionnaire should go to all the places he has sent the previous request for information and in addition, the other places where Bockris claims that tritium has been observed, Florida and Case Western. At Texas A&M separate questionnaires should go to Appleby, Bockris, Martin and Wolf.

I will be leaving for Japan on 22 September and will return during the week of 13 October. Therefore, I would like to get responses on or before 1 September.

Cordially,

Jake

Jacob Bigeleisen

xc: W. Woodard

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7 August 1989

Dr. Reed Jensen,
Deputy Associate Director for Research,
Los Alamos National Laboratory,
P.O. Box 1663 MS-A114
Los Alamos, New Mexico 87545

Dear Reed,

The ERAB Panel on Cold Fusion is preparing its final report. There have been word of mouth reports that tritium has been found at Los Alamos in electrolytic cells in which solutions of LiOD in D_2O are electrolyzed with Pd cathodes. I am writing as a panel member, specifically assigned to look into claims of tritium production, to get definitive information on the Los Alamos experiments. Whatever information you can supply will be appreciated. I would like you to address the following points, inter alia:

1. source and specific activity of D_2O used in the electrolytic cells.
2. description of the electrolytic cell, including volume of D_2O.
3. cell operating conditions.
4. schedule of additions of D_2O during operation of the cell.
5. Tritium analyses:
   a. method of purification of samples for analysis
   b. analytical method
   c. schedule of cells examined for possible tritium production
   d. specific activity of D_2O, $D_2(g)$ and PdD_n as a function of time for each cell
6. Neutron production rate in each cell or in comparable cells.
7. Heat balance measurements in each cell in which tritium is found or in comparable cells.
8. What tritium sources are located in the buildings where the electrolytic cells are operated and/or in the analysis stages.

Any additional pertinent information relative to possible tritium production will be appreciated.

Professor John O'M. Bockris of Texas A&M has reported that the electrolyte from one of his cells has been analyzed for tritium at LANL amongst a total of six laboratories. I do not have a report on the Los Alamos results. Could I get a report on the Los Alamos result, a summary of the history of the sample as it arrived at Los Alamos, a description of the procedure used at Los Alamos to purify the sample prior to analysis, and the method of analysis.

I appreciate the fact that you will be making an official report to ERAB on or before 15 September. My task will be greatly simplified if I could get a report from you on the tritium on or before 1 September. I thank you for your cooperation in this matter.

Cordially,

Jacob Bigeleisen

Jacob Bigeleisen
Distinguished Professor Emeritus

xc: J.R. Huizenga
W. Woodard
FAX 505-665-3858

Date: Fri, 4 Aug 89 14:41 EDT
From: <JBIGELEI@SBCCMAIL>
Subject: Menlove's Neutron Bursts
To: rlg2@yktvmv
X-Original-To: rlg2@yktvmv.bitnet

State University of New York at Stony Brook
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Jacob Bigeleisen
Professor
Chemistry
516-632-7905
04-Aug-1989 02:33pm EDT

FROM: JBIGELEISEN
TO: Remote Addressee (_RLG2@YKTVMV.BITNET)

SUBJECT: Menlove's Neutron Bursts

Dear Dick,

I have read your recent exchange with Howard Menlove of Los Alamos. You will be interested in a recent conversation I had with Maurice Goldhaber. Maurice, who is skeptical of Menlove's bursts, suggested to Menlove that he replace the gas in half of his counters with 4He. What do you think Menlove answered. The counters are commercial counters and are sealed. Apparently he doesn't know how to build a counter or is releuctant to order a set filled with 4He. Menlove's results have more significance that what confusion they add to the cold fusion story. What is his primary mission and what confidence can we have in his results? JAKE

Energy Research Advisory Board
to the
United States Department of Energy
1000 Independence Avenue, S.W.
Washington, D.C. 20585
(202) 586-5444

August 2, 1989

To: Cold Fusion Panel

Enclosed for your information is the final IPP report on cold fusion efforts in Garching.

William Woodard MR

William L. Woodard
Panel Secretary

Enclosure

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July 26, 1989

Dr. John R. Huizenga
Co-Chairman
Cold Fusion Panel
US Department of Energy
ER-6
1000 Independence Av., SW.
Washington, D.C. 20585

Dear Dr. Huizenga,

Enclosed you will find a copy of our final IPP report on the topic of our past cold fusion efforts in Garching. The report summarizes our findings on the topic...with particular emphasis towards the University of Utah results. We were not as excited about the claims by Jones et al, and did not have the inclination or quick capability to test his results. We basically agree with the interim recommendations of your panel, in the draft report, and hope that our report IPP III/149 "Electrochemical *Cold Nuclear Fusion* Trials at IPP Garching", by Bosch, Wurden, Gernhardt, Karger and Perchermeier is helpful to your panel in some way. Incidently, we are a group of both physicists, and chemists.

We also hope that the eventual outcome, from the standpoint of damage to the public's view of fusion in particular, and science in general, can be minimized. Damage between the chemistry and physics community, which was caused by the abomination of the ACS Los Angeles meeting requesting only talks which *supported* CNF, will *only* slowly be healed. Ideally, the end result will be a better appreciation by the public on how good science is (or ought to be) conducted, and taxpayer's dollars will not have been completely wasted. We are all now back to work on our hot fusion experiments, after a fun, but disappointing interlude.

Sincerely,

Glen Wurden

Dr. G. A. Wurden
Los Alamos National Laboratory
DOE/ASDEX-EURATOM
Exchange Scientist

MAX-PLANCK-INSTITUT FÜR PLASMAPHYSIK

GARCHING BEI MÜNCHEN

Electrochemical "Cold Fusion" Trials at IPP Garching

H.–S. Bosch, G. A. Wurden*, J. Gernhardt,
F. Karger and J. Perchermeier

IPP III/149 July 1989

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Die nachstehende Arbeit wurde im Rahmen des Vertrages zwischen dem Max-Planck-Institut für Plasmaphysik und der Europäischen Atomgemeinschaft über die Zusammenarbeit auf dem Gebiete der Plasmaphysik durchgeführt.

ELECTROCHEMICAL COLD FUSION TRIALS AT IPP GARCHING

H.–S. Bosch, G. A. Wurden, J. Gernhardt,
F. Karger, and J. Perchermeier

IPP III/149

Abstract: Following the report of Fleischmann and Pons [1], we (The Bavarian Bubble Bottle Team) have attempted to reproduce their claims of cold nuclear fusion, and failed. We note that our measurements would not be able to detect neutrons at the level of Jones *et al.* [2]. Three electrolytic cell experiments were conducted using palladium cathodes and Platinum anodes, in a 0.1 Molar solution of LiD in heavy water, without any signs of neutrons, tritium or gammas above backgrounds, and within ± 0.3 Watt accuracy calorimetry, no excess heating. Excess heating at the levels of F&P would have been easily detected, if present. Intrinsic tritium, differing from each D_2O bottle tested, was however observed. The longest duration experiment ran for 21 days, and was an attempt to duplicate the large "melting incident" of F&P. This was terminated on April 28, 1989, by throwing the vacuum-cast 22 gram, deuterium-loaded palladium cathode directly into liquid nitrogen, immediately next to a bare BF_3 counter (backed by 25 cm of moderator), in order to attempt one of the Italian ENEA neutron production variants. No neutrons above backgrounds were seen, while counting for one hour, and also none while the piece warmed to room temperature over the next hour. Post mortem analysis of the darkened, hardened Pd piece showed large crystal grains (up to 2mm×2mm), and continuing evolution of gas bubbles at the grain boundaries even four days after the experiment was ended. Eight-weeks after loading, the catalytically active palladium piece continued to create heavy water (with exposure to oxygen in the air).

Contents

1 Introduction

During the long holiday weekend of Easter, 1989, we first began to hear radio and TV reports of "fusion in a test tube", originating from the University of Utah. Most of us rejected the brief news reports as quackery and fluff. A televised video of a laboratory scene with a bubbling cell, without any heavy shielding, caused us immediately to ask "If this could be real, why aren't the experimenters dead from exposure to radiation?", which would accompany nuclear fusion in quantities to account for the claimed (a few Watts) energy releases.

Then, with rumors beginning to fly across computer networks and faxes, we received the reports of "vaporized electrodes", and that one of the principals (MF) was a respected Fellow of the Royal Society, and the other (SP) was head of the U of Utah chemistry department. Still with considerable doubt and a lot of skepticism, mixed with some wonderment if it just might really be possible... a few of us begin to look around in the IPP for the necessary chemicals, detectors, and a safe place to operate. We conducted a basic library search on properties of palladium and metal hydrides, and began to interact with metal hydride experts at the Technical University...who indicated that Fleischmann should be taken at his word. Furthermore, we were certain that a pair of chemists would have done careful calorimetry, as this is a basic tool of the physical chemist's profession. We felt that calibration of the nuclear measurements might be another question, but this didn't concern us greatly at the time. *Any* chemical process driving a reaction creating nuclear products is in of itself, interesting.

Our only initial "engineering" information was the March 23, 1989 "Financial Times" article [3], which had a crude diagram, and a March 25, 1989 article in "Die Welt" newspaper showing a picture of Fleischmann holding one of his cells in his hand. This information was dispersed at the highly publicized initial press conference announcement. Conspicuously missing from these accounts were the electrolytic "salt" and concentrations used. We gathered more articles, and discussed possible lines of action among our self-assembling team on Tuesday March 28 (Monday was an Easter holiday). We turned on our first experimental cell on Thursday afternoon March 30, 1989. Minor delays for machining appropiate glass cell fittings, and setting up the necessary radiation detectors, chart recorders, etc., in a fume hood of the electrochemical plating laboratory of the institute, progressed in parallel with information gathering.

It was obvious from the beginning that the F&P experiment was either going to be one of the biggest flops of the year, or else one of the most important discoveries in this century. Since our interest in achieving fusion energy in a controlled fashion for the production of energy is not necessarily tied to any one technique, (ie, the tokamak), we felt it important to verify these astonishing claims, if possible. If something unexpected might be occurring in a lattice of palladium fully loaded with deuterium, that just maybe no one else had noticed before, we wanted to *know*.

Discussions with Los Alamos, Princeton, and Harwell Laboratories, told us that other people were also taking the claims seriously. As it has been stated in the press, "the race

was on". Computer bulletin boards were critical for early accounts of seminars, or phone discussions from the Utah principals involved. Through these means, we first found out to use lithium as the electrolytic solute, and that it might be important to minimize exchange of H_2O from the air with D_2O in the cell. Over the weekend of April Fool's, we continued to sort and assimilate the wild speculation floating around on the computer networks.

2 Properties of the Pd/D/Li System

For the purposes of this report, we describe here some of the relevant information on the extremely complicated Pd/D system, which is complicated even further by the addition of lithium, which is also soluble in palladium. We make no attempts at completeness, for entire books are written on differing aspects of the subject[4,5]. Since we are a team of physicists and chemists, right away we found it convenient to make a small table of conversion factors, which is reproduced here as Table 1, so that we could all speak the same language. Tritium data comes from Sauter[6].

Table 1: Useful conversions & constants for Chemists and Physicists

1 cal	=	4.184 Joules
1 kcal/mol	=	0.04335 eV/molecule
1 eV/molecule	=	23.069 kcal/mol
1 Faraday of charge	=	96487 Amp-sec
1 Mol T_2	=	2.15×10^{15}Bq
1 dpm/ml	=	1 decay/minute/milliliter
1 dpm/ml	=	1.66×10^4 Bq/m^3
Tritium in rain(1973)	=	1.1 dpm/ml

Palladium, a silvery colored noble metal, has a density of 12.0 gm/cm^3, an atomic weight of 106.4, a melting point of 1554°C, is known for its catalytic ability, and capability of absorbing large amounts of hydrogen (900-2800x its own volume in H_2). The hydrogen-poor state is referred to as the α phase, and the hydrogen-rich state, at about a 0.5 D/Pd ratio, is the β phase. Overloading to ratios between 0.8-1.3 D/Pd, where the deuterium is thought to continue accumulating in crystal voids, is also reported to be possible with high electrolytic current densities ($\sim$ 1 A/cm^2). Palladium metal consists of a face-centered cubic lattice, with four atoms in the elementary cell. The "empty" α-phase crystal constant is $a = 3.88$Å, with the nearest neighbor distance of $a/\sqrt{2}$. The crystal lattice expands approximately 5% as hydrogen is absorbed. There is considerable uncertainty in the literature, as to the exact nature of the bonding sites of hydrogen in palladium.

It has also been reported for more than 125 years, that a palladium piece, loaded with hydrogen, when removed from solution and placed in the air, may in a short time *spontaneously heat to a red, or orange-hot glow* and can set filterpaper *aflame* or even "explode". This effect depends strongly on the previous surface conditions of the palladium, and its prior history of "loading/unloading" cycling. This effect is a result of catalytic recombination of the hydrogen with oxygen, and nearly the entire load of hydrogen can be rele[illegible] the runaway heating reaction, over a brief timespan[4]. As early as 1823, a man named Döbereiner invented a cigarette lighter (the so-called "Döbereiners Feuerzeug") using a similar effect of hydrogen on platinum.

The actual loading of D_2 into Pd is exothermic, and endothermic when the deuterium unloads (if no further catalized reaction with oxygen occurs). Numbers for H_2 gas loading of the α-phase are 6-8 kcal/mole H_2, followed in the β-phase by 9.7 kcal/mole H_2, with an integrated value of 8.7 kcal/mole at 30°C, have been reported[7]. The heat of absorption for deuterium is reported by Lewis as being about 15% smaller than for hydrogen. Furthermore, the absolute values of the heat of absorption climb with increasing hydrogen loading into the β-phase. However, additional reactions (such as atomic recombination of D° to D_2, or formation of water) at the surface of the Pd may dominate the net kinetics. Some of these reactions for hydrogen are shown in Table 2 below.

Table 2: Some relevant chemical reactions

$H_2O_{(l)} \rightarrow H_{2(g)} + 1/2\ O_{2(g)}$	+68.4 kcal/mole(H_2)
$D_2O_{(l)} \rightarrow D_{2(g)} + 1/2\ O_{2(g)}$	+70.2 kcal/mole(D_2)
$H_2O_{(l)} \rightarrow H_2O_{(g)}$	+10.76 kcal/mole
$D_2O_{(l)} \rightarrow D_2O_{(g)}$	+11.15 kcal/mole
$H_{2(g)} + 2\ Pd \rightarrow 2\ PdH$	-8 kcal/mole(H_2)
$H_2 \rightarrow 2\ H$	+104 kcal/mole(H_2)
$2\ H + 2\ Pd \rightarrow 2\ PdH$	-112 kcal/mole(H_2)

Hence in any case, one *should* see a thermodynamic difference between a heavy water and light water test cell, without necessarily invoking any nuclear processes. Consequently, the publicized persistence of R. A. Huggins[8] (Standford) of reporting heat differences between his heavy and light water cells, is at first glance plausible, depending on the nonequilibrium conditions in his cells.

2.1 Estimate of loading times

To give the reader an idea of the absolute minimum timescales for loading a piece of palladium electrolytically, let us consider a hypothetical 1 cm^3 piece, containing 6.8×10^{22} atoms of palladium. If a current of 2.5 Amps was 100% efficient at loading a D^+ ion into the lattice, allowing no extraneous bubbles, essentially instantaneous diffusion rates, and

no other recombination effects, while neglecting the lithium, then the loading time for a 1:1 fraction of Pd:D would be approximately 1.2 hours. This only serves as an absolute lower limit.

In reality, the diffusion velocity of deuterium through palladium depends on the surface conditions of the palladium, and its prior history. At current densities of order 10 mA/cm^2 or higher, deuterium can not diffuse into the metal fast enough, and the excess appears in the form of bubbles on the cathode. Hence for a cubical configuration, identifying the useful current density as only 10 mA/cm^2, one might expect a loading time of approximately 50 hours.

It is believed that the loading time scales with the thickness of the piece, squared[4]. The e-folding diffusive time constant is of the form $\tau = r^2/\pi^2\Delta$, where r is the radius of the Pd rod, and a diffusion rate (pessimistically) $\Delta \sim 10^{-7}$ cm^2/s. Approximating the cubical case above, then one obtains by this estimate a time of 70 hours. More detailed measurements of the α and β-phase diffusion rate constants at room temperature, show the β-phase to have about a $10 - 30\times$ higher diffusion rate at room temperature[5].

As a rod of palladium is loaded with deuterium, the β/α transition zone slowly sweeps from the edge into the center of the rod[4]. Existence of neighboring metal crystals in different phases has complicated crystallographic determination of the loaded-lattice constants. Under electrolytic-loading at 1.6 mA/cm^2, it has been observed that the phase boundary advanced upwards out of the surface of the solution, in a piece of Pd only partly submerged, at a velocity of 0.2 mm/hour[9]. This implies loading times for a 5 mm radius piece would be greater than 25 hours.

Summarizing this section, the deuterium loading time for a 5 mm radius Pd piece is estimated to be of the order of 30-90 hours at a current density of 10 mA/cm^2 or higher, if the surface isn't chemically poisoned.

2.2 Comments on heat balance

Observations of "excess heating", or changes in the heat balance of the system, after hours of stable operation, must be extremely carefully considered. In particular, changes *are* expected after the palladium saturates with deuterium, because of differences between when deuterium is lost in the bubble form at the surface of the palladium, as opposed to when the same deuterium is loading into the palladium. In addition, whether or not the dissolved deuterium is effectively in atomic or molecular form as it is bound in the lattice, or even later, simply in interstitial cracks, all affect the net heat balance. This doesn't even include possible Li-D, or Pd/Li reactions...and is only discussed above for the part of the palladium immersed in water! If for some reason a portion of the palladium becomes catalytically activated above the liquid surface during the course of the experiment, then considerable heat is released from the catalyzed recombination of the electrolytically disassociated heavy water, instead of being counted as a loss to the system. This effect is typically of order 10-50% of the total work delivered to the cell (depending on the cell voltage and current density), and can explain the so-called

#	Nuclear reaction	Q-value (MeV)	specific reaction rate ($s^{-1}W^{-1}$)	Ref.
1	p+d $\rightarrow\gamma+^3$He	5.49	1.13×10^{12}	[12]
2	d+d $\rightarrow$p+t	4.03	1.54×10^{12}	[13]
3	d+d $\rightarrow$n+^{3}He	3.27	1.90×10^{12}	[13]
4	d+d $\rightarrow\gamma+^4$He	3.85	2.61×10^{11}	[14]
5	p+t $\rightarrow\gamma+^4$He	[illegible]	3.14×10^{11}	[12]
6	d+t $\rightarrow$n+^{4}He	[illegible]	3.53×10^{11}	[13]
7	d+t $\rightarrow\gamma+^5$He	16.63	3.74×10^{11}	[12]
8	p+^{6}Li $\rightarrow^3$He+^{4}He	4.02	1.55×10^{12}	
9	d+^{6}Li $\rightarrow^4$He+^{4}He	22.4	2.77×10^{11}	
10	p+^{7}Li $\rightarrow^4$He+^{4}He	17.5	3.55×10^{11}	

Table 3: A (short) list of nuclear reactions discussed for cold fusion.

"excess" heat output of the F&P cell in every published case[10].

2.3 Comments on nuclear processes

Here we quickly review some information about known nuclear reactions which might possibly be involved in the so-called cold fusion experiments. First news reports talked about powers of the order of "Watts" from nuclear reactions. With a conversion factor of 1.609×10^{-13} Joules per MeV we get the specific reaction rates (reactions per second per Watt) listed in Table 3. The energies released with these reactions have been calculated from the atomic mass excesses as listed in [11].

The reactions (2) and (3) are those used in existing hot fusion plasma experiments and their cross-sections are roughly equal. The thermal reaction rate in a deuterium molecule for these reactions is in the order of 10^{-64} reactions/second/particle pair[15]. Although for reaction (4) the cross-section is a factor of about 10^{-7} lower[16], it received a lot of attention since it liberates large amounts of energy, with no direct neutrons. However there has been no convincing idea of how to overcome its small cross-section or how to deal with the absence of the normal, powerful (23.8 MeV) gamma-ray, although speculation did center on "coherent processes" (P. Hagelstein, MIT).

Almost all the nuclear reactions in this table create a helium isotope, and therefore the detection of He in the Pd samples would be a significant indication of fusion, and conversely, absence of helium in a "working cell" would weigh heavily against fusion as the source of any "excess heat". Widely publicized reports concerning the presence of high levels of helium (Wallings and Simons, Utah) were withdrawn, and a cell from Texas A&M which *is* reported as having excess heat, has much too low a level of helium to associate the excess heat with nuclear fusion[17]. For those reactions which don't

create directly gammas, the generation of fast charged particles would create secondary gammas due to bremsstrahlung or from Coulomb excitations of palladium nuclei[18]. The idea of a nuclear-equivalent Mössbauer effect, where up to 24 MeV of reaction energy could be absorbed by the metal crystal lattice through the generation of thousands of phonons, without accompanying gammas, is highly exotic in our opinion. In summary, we know of no nuclear fusion process, which would not be accompanied by the generation of detectable amounts of nuclear radiation of some sort!

3 Experimental Setups

Following information gleaned from news reports, newspaper photos, computer network mail, we began experiments in a chemical fume hood, on the second floor of the chemistry lab of the IPP. Concern about possible chemical explosions from recombination of the deuterium and oxygen electrolysis products required a location with good ventilation. A location with good radiation shielding from a health/safety viewpoint was only a secondary consideration, since we didn't have any good reason to believe in hazardous radiation production, but we were certain about the chemical hazards. From the viewpoint of shielding against background radiation in order to see lower signal levels, we were confident that we could easily see levels as large as P & F were initially reporting, without special precautions. (Had we started our experiments later, we would have, in retrospect, gone for better background shielding.)

The form, and amount of palladium immediately available for experiments was initially a limiting factor. We had several grades of heavy water, and a variety of deuterated salts and acids available. Platinum mesh anodes, and sheets of platinum-irridium were also available. We had both metallic lithium (in kerosene) and pure LiD powder. For the second and third experiments, 99.75% purity D_2O was used, originating from a total of three 100 ml, and one 250 ml Merck flasks, all more than 12 years old. The third experiment used a low purity D_2O water bath for a time, in order to minimize possible H/D exchange to the high purity central cell over extended periods of operation. Towards the end of the third experiment, this was replaced with a normal H_2O water bath, and insulation of the water bath was modified to reduce evaporative losses and isolate heat from the magnetic stirrer motor.

Thermo-resistor elements and multi-pen strip chart recorders were borrowed from ASDEX, as well as direct readout thermocouple elements. Since the experiment would run for long periods, without constant human monitoring, the "paper tape" readout was essential. Standard precision ($\pm 0.1°C$) thermometers were also placed for "human readout" as needed, in both the center cell, water bath, and as an air temperature indicator. We looked for a constant temperature water bath regulator with known cooling/heating inputs, but did not find one to suit our needs, and hence ended up using a water bath cooled by a constant flow of room air. The cell current, voltage, air temperature, and water bath temperature were continuously monitored on a 4-pen strip chart recorder.

The temperature scale was 4°C/cm, with a reading accuracy of 0.5 mm, corresponding to 0.2°C, comparable to the additional manually read thermometers.

NaI detectors, normally used for measuring radiation produced by runaway electrons in ASDEX, were borrowed from Dr. Fussmann. A 2" diameter, 10" long BF_3 detector, encased in a polyethlene moderator, was our main neutron diagnostic. We borrowed an automatic, absolutely calibrated radiation monitoring system normally used for safety measurements in the ASDEX building. A pulse height analyzer was used for both gamma spectra from the NaI detector (later also a Ge(Li) crystal), and to observe the proper pulse energy from the large BF_3 counter.

3.1 Exp. #1, Thin-wall Pd tube

Our first attempt principally served as a "shake-down" experiment for later efforts. This experiment was performed with materials at hand, and stock chemistry glassware. A 3.85 gm thin-walled palladium tube (1.5 mm dia, 0.2 mm wall, 19.4 cm length, outgassed in a vacuum oven at 300°C overnight) was coiled in a vertical spiral, with both ends of the tube extending above the surface of the electrolyte. The underwater portion had a length of 15 cm, for a useful loaded-volume of 0.25 cm^3. A 26 gm cylindrical platinum mesh anode surrounded the cathode, and the cell was a 250 ml Pyrex beaker, surrounded by a stirred 800 ml water bath.

This run began on Thursday afternoon, March 30, before we had gotten any information about the desired electrolyte makeup or concentration. We only had vague rumors about lithium, and we fortuitously chose to use 100 mg of LiD per 100 ml of D_2O, which turned out to be almost exactly what F&P used in their experiments (0.1 Molar)! Low-grade heavy water (95% isotopic purity) was used (only in experiment #1) as the solvent to makeup the electrolyte. Later, we continued to use LiD instead of metallic lithium, because the heat of reaction upon solvation is lower for LiD, and, in addition, we didn't have to worry about removing kerosene or oil from the available metallic lithium. In particular, we didn't want to contaminate our electrolyte with any hydrogenated compounds.

3.1.1 Evolution of bubbles

With this first attempt, we watched (as the cell was initially turned on) the delay between bubbles first appearing at the anode, and then 1-2 minutes later (at 130 mA and 3 volts) bubbles appearing on the surface of the palladium cathode... indicative of the initial absorption of deuterium into the palladium lattice. The chosen operating point was 1 Ampere, and a voltage of 5.4 Volts, which with the 2.5 cm^2 surface area (one-sided), corresponded to a 400 mA/cm^2 loading current. We noticed a large, continuing flux of gas (presumably deuterium) escaping up the center of the cathode, and realized that since the inner wall of the cathode was *not* exposed to electrolysis pressure, that it was unlikely we could "overload" the Pd with deuterium. Hence the run was stopped after

an overnight vigil, and a duration of 16 hours. (A 10 minute interruption in the current after 7 hours was made to watch the bubble release from the cathode, and observe the voltage decay). The mass increase of the cathode was 40 mg, or if all deuterium, this corresponds to a ratio of 0.7:1 D:Pd atoms. We later made estimates of the loading timescale to be longer than 45 minutes.

3.1.2 Temperature(s)

During this first experiment only, one of the two (strip-chart monitored) thermo-resistors was encased in a thin glass capillary tube, and actually immersed directly in the center cell. Water in the capillary conducted heat to the metal probe jacket. A meter-readout thermocouple element was similarly encased but measured the center cell temperature in the solution, near its surface. (The reason for the glass jackets, was to avoid unwanted electrolysis action on our sensing elements.) First crude open-cell heat balance

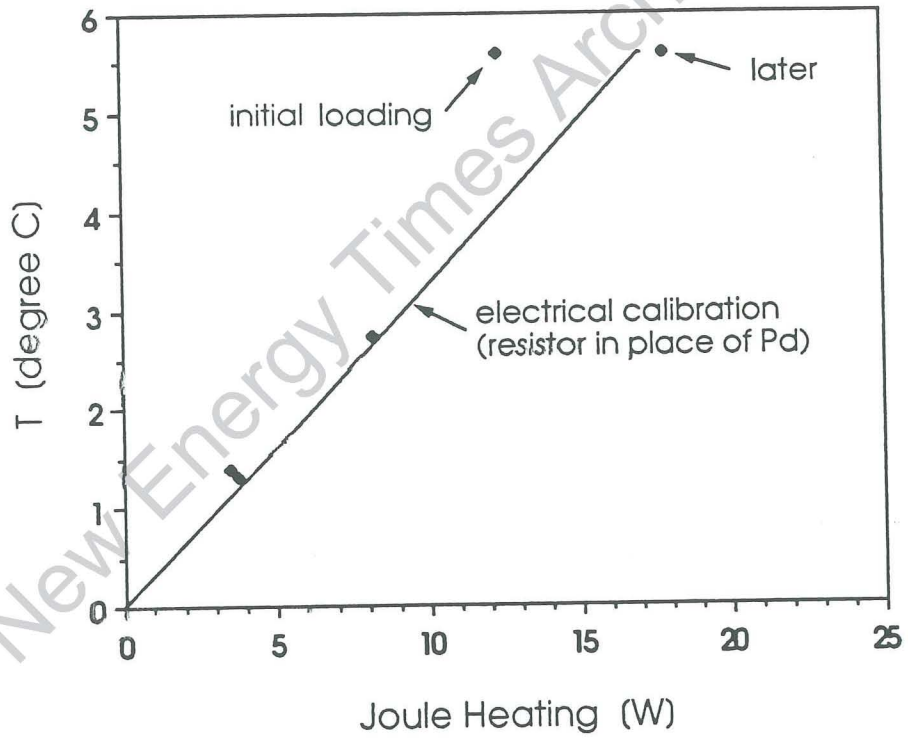

Figure 1: *Experimental temperature measurements in the # 1 cell. Temperature rise of TC at bottom of center cell relative to the water bath. Time constant is only 3 minutes. The cell was hotter during the first 10-15 minutes during the loading phase, than hours later in equilibrium.*

measurements are shown in Figure 1. As a zeroth order approximation, a 5.5 Ω resistor was used for comparative ohmic heat inputs, in the position of the Pd (but without bubbles). With the actual Pd/Pt cell, at its nominal operating point of 5.4 Watts, one could measure a 2°C temperature differential between the top and bottom of the cell, along

a vertical chord just *outside* the Pt anode mesh. Near the Pd itself, the gradient was even larger. We calculate the Joule heating from the electrolysis current, by subtracting 1.54 volts from the applied voltage (as F&P have done). Initial nonequilibrium effects of the exothermic loading was apparently observed, but clearly better measurements (in the following experiments) were needed to draw more significant conclusions.

Even though we noted "strong" bubbling, it was quickly clear from multipoint measurements with the three thermometers directly in the center cell, that a "single point" temperature reading in the center cell would not be representative of its energy content, due to substantial thermal gradients. We did not know at that time, that this was precisely what F&P had done.

3.2 Exp. #2, Large surface area Pd sheet

The second cell was put into operation on April 4, 1989, after our first receipt of the F&P paper on Monday evening April 3.

This cell was more sophisticated, and some parts actually took a day in the shop to prepare! A 1/2-scale sketch of the cell is shown in Figure 2.

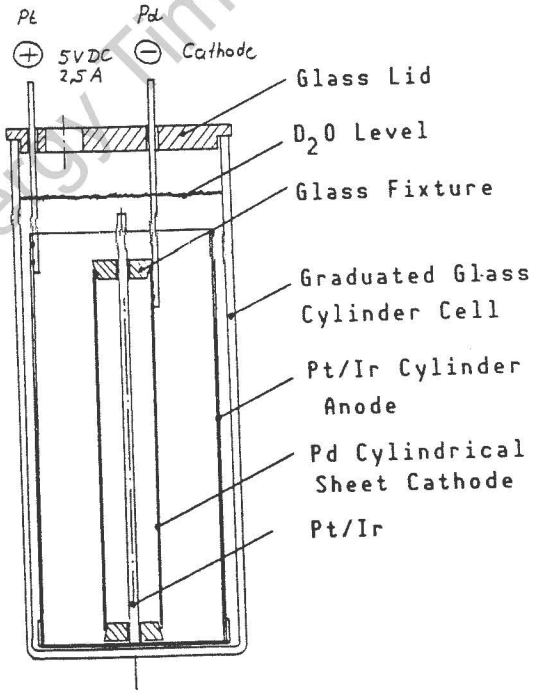

Figure 2: *Schematic of the second experiment, showing concentric Pt/Ir anode, and inner palladium cylindrical sheet, with central Pt/Ir wire, to insure a well-defined field pattern on the 0.2 mm thick palladium sheet. The hole in the glass lid was plugged with a conventional glass-encased mercury thermometer.*

We still hadn't located any solid rods of palladium, so we employed a cylindrical thin sheet of palladium as the cathode. A two part anode, with a central Pt wire, as well as a

concentric surrounding Pt/Ir sheet, allowed both "sides" of the palladium to be subject to electrolysis. The electric field configuration was well-defined, though undoubtedly giving a differential current flux on the two surfaces of the palladium sheet. The sheet was 0.2 mm thick, 9.4 cm tall, and had a cylindrical diameter of 1.5 cm, for a mass of 11 grams. Pretreatment of the rolled sheet was limited to scouring the surface with a wire mesh, followed by vacuum-baking overnight at 300°C. The total (two-sided) surface area of 88 cm^2, was designed to be completely submerged in solution and a thin-walled Pd (1.5 mm dia) tube formed the spot-welded electrical connection to the cathode. Losses out the top of the cell were restricted to small holes in the close-fitting glass lid.

Initially to get an idea of the chemical purity of the 99.75% D_2O, we turned up the cell voltage (before adding the LiD salt) to a value of 48 volts, obtaining only 60 mA of current. Then after the salt was added, bringing the concentration up to 0.11 Molar, we raised the current up to a value of 2.5 amperes, at a voltage of 5.5 volts.

Based on our #1 experiment, we placed the chart-recorded thermo-resistors in the stirred water bath, and in the air. Air flow was maintained constant in the fume hood, by always keeping the fume hood door in a fixed position. With an air temperature of 24-25° C, the stirred water bath was in equilibrium at 34° C, and a thermometer in the center cell read 36.5° C near the bottom of the center cell. At a current of 2.5 Amps and a voltage of 5.0 volts, the Pd had an average current density of 25 mA/cm^2. This was maintained, with minor interruptions (to watch the bubbles unload at one point, and at another, to repair a bad tack weld contact mechanically) for 45 hours. After the first four hours of operation, thermal conditions in the cell were essentially stationary for the remaining 41 hours of the run. Upon completion of the run, a 2 ohm resistor was put in place of the palladium, to crudely simulate the ohmic heating (although admittedly without the effects of bubbles), directly in the center cell, while monitoring power, air and bath temperatures with the strip chart recorder.

3.3 Exp. #3, Cast Pd stone

After our initial searches for palladium yielded only thin-wall palladium tubes from hydrogen purification systems, and 0.2 mm thick sheets of palladium from purification fingers, we realized we needed to buy, or make our own pieces. Since the time for purchasing palladium was long (about a month), we took pieces of palladium we had, and looked for a suitable furnace, preferably in a vacuum. The electroplating group at IPP also has a vacuum apparatus with an e-beam for melting metals. We thought this would be satisfactory, and we could also pump away desorbed gases.

Using this facility we created a 22 gram vacuum-cast (1.8 cm^3) "stone", to reproduce the so-called "ignition?" experiment of F&P, as shown in Figure 3. Due to the available water-cooled copper crucible, the shape was irregular, much like a pebble that you would choose to skip across the water at a stream. A large quantity of gases were evolved during the 10-minute melting process. No machining was done on the piece, which was allowed to cool gradually to room temperatures in vacuum. The resulting shiny silvery piece had

a surface area of 10 cm^2, with the long dimension of 2.8 cm, thickness of 5-6 mm, and a width of 1-1.4 cm. A thin-hollow palladium tube (approximately 2 gms worth) was also attached with the e-beam, to serve as the electrical lead, and mechanical support to the stone. During later operation, the stone was fully immersed, with its top approximately 1.5 cm underwater.

The reactor then consisted of a machined 200 ml pyrex beaker, with a close fitting glass lid, having four small holes, one for a thermometer, one each for the palladium and platinum electrical connection. A cylindrical platinum mesh anode surrounded the palladium cathode. Gas and a small amount of spray could escape through two of the holes (as evidenced by a gradual white powder residue build-up, known to be Li_2CO_3, from carbon dioxide in the air interacting with the strongly basic LiOD solution). A photo depicts the preassembly in Fig. 3, and a ruler is shown for scale.

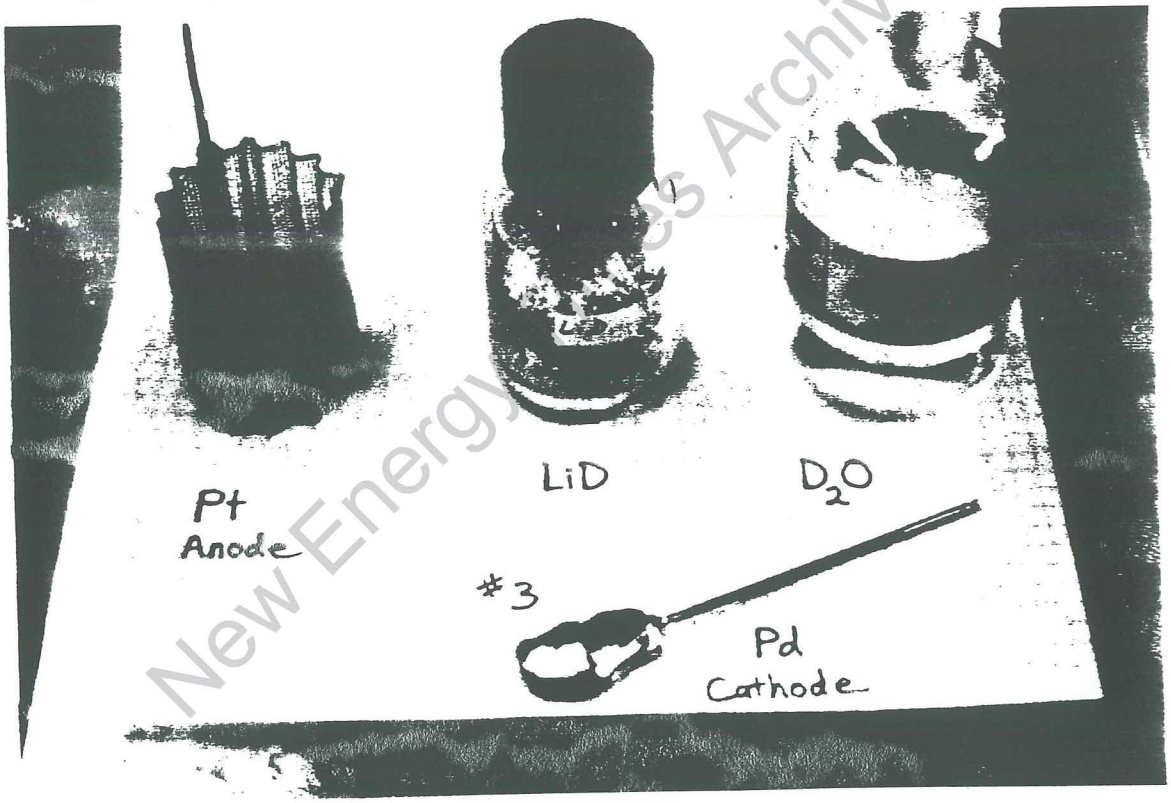

Figure 3: *Picture of the third, and longest running experimental cell components, including from left to right, the Platinum mesh anode, the bottle of LiD powder used, the Merck 99.75% heavy water, and the e-beam melted 22 gram palladium "stone".*

The assembly was then supported in a larger 1-liter beaker, which was filled at first with 750 ml of low grade D_2O, and then later in the run with the same amount of normal water. A thermo-resistor monitored the bath temperature in the middle of the bath, and a digital readout thermometer monitored the surface temperature of the water bath.

A photo of our final working setup in the lab is shown in Figure 4a, and a closeup of the #3 cell in operation in Fig. 4b. In the overview picture, the pulse height analyzer

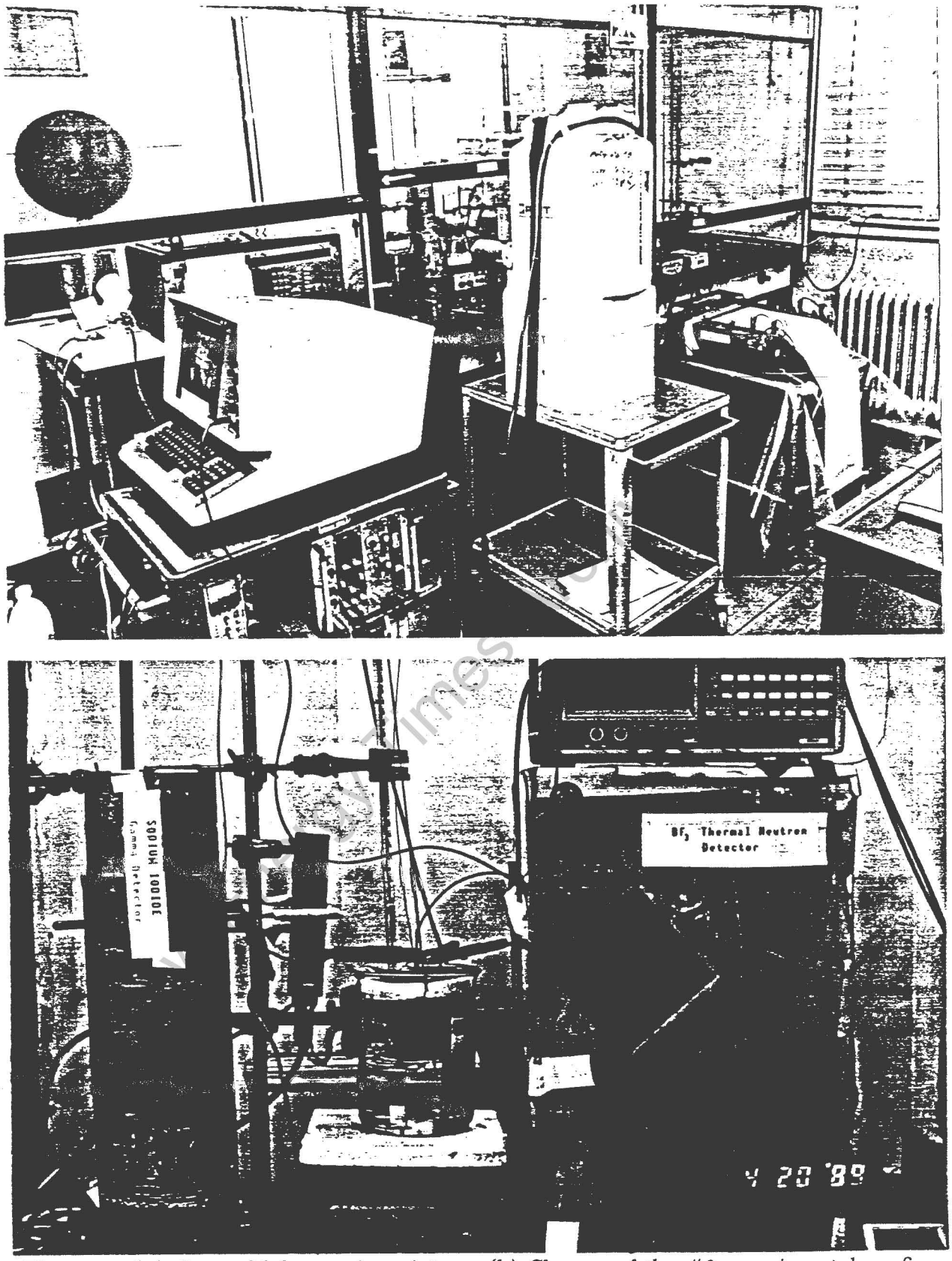

Figure 4: *(a) General lab overview picture. (b) Closeup of the #3 experimental configuration on April 20, 1989.*

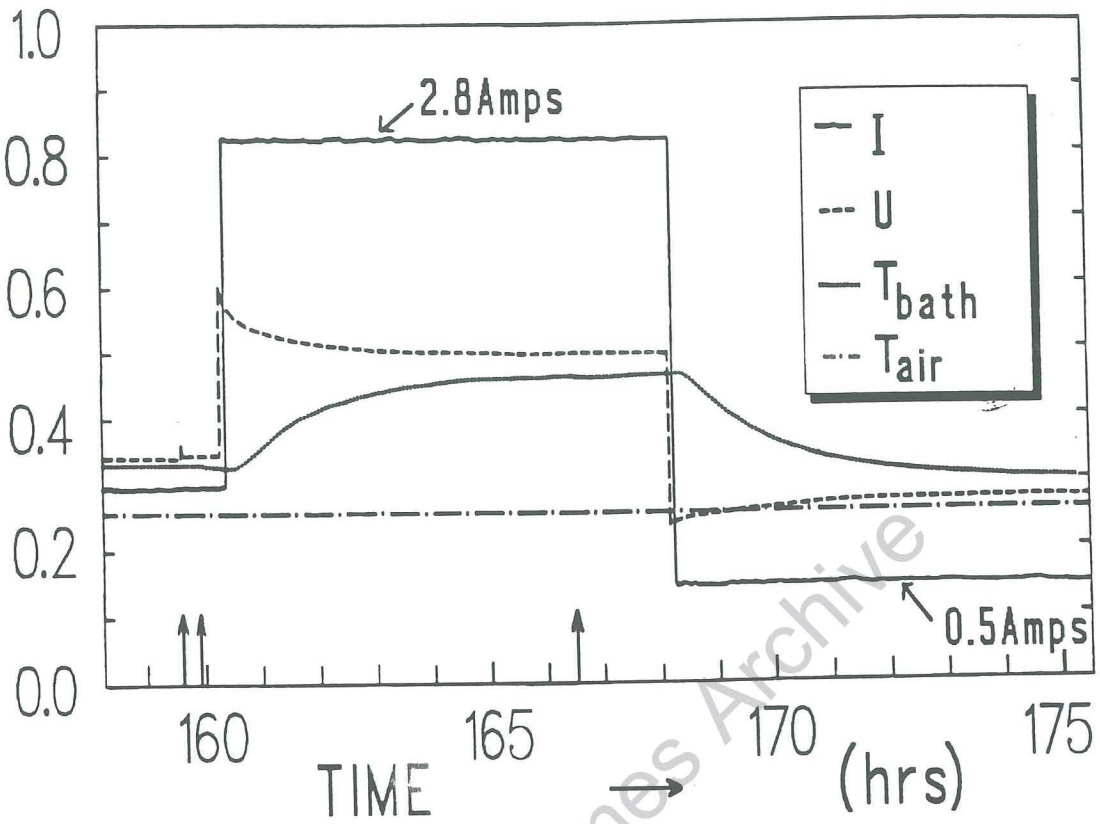

Figure 5: *A sample of the multi-pen chart recorder traces during forced transients, 160 hours into the #3 experiment. Cell current, voltage, and waterbath and air temperatures from the strip chart record are shown for a 17 hour period. Full scale readings correspond to 3.4 Amps, 18.8 Volts, and 100 °C for the respective traces.*

and chart recorder are visible in the foreground, while the cell itself is almost hidden by the Princton GammaTech Ge(Li) high resolution gamma detector. The Bonner sphere of the absolutely calibrated Berthold monitoring system, and controller, sits in the left fumehood. In the closeup photo, one sees the lead-shielded NaI detector to the left, the water bath of the cell in the middle, and the rectangular shielding of the BF_3 neutron detector to the right. Insulation has been added between the stirrer motor and the waterbath, and also some floating insulation on the waterbath itself, to further reduce long term evaporation. Thermometers and thermal-resistor probes are also visible.

Typical loading currents were 2-2.5 Amperes, with a current density then in the neighborhood of 200-250 mA/cm^2. The surface of the stone was noticeably darkened after a day of operation. The current was maintained for constant for approximately 145 hours, before being changed as an adjustable parameter over the subsequent 360 hours of operation. Rapid current increases from 1 A to 3 Amp were peformed, and also rapid decreases down to 0.5 A. After waiting for several 100 minute e-folding times of the water bath, new readings would be taken. An example of such changes are shown in Figure 5, where the response of the voltage and bath temperature are recorded on the

strip chart, in addition to the controlled current and monitored air temperature traces. Perturbations due to topping off the cell with room temperature D_2O, and of refilling the water bath to its marked height, are also visible and denoted with arrows. Replenishment of the center cell D_2O averaged 16 ml/day. No unexplained excursions were apparent when we tried to force nonequilibrium conditions.

3.1 Termination: "Fusione superfredda" attempted

We stopped the #3 experiment after 21 days of operation, by shutting off the cell, disconnecting leads, and quickly (in 10 seconds) dropping the loaded Pd piece directly into a Dewar filled with a few hundred ml of liquid nitrogen. The Dewar was surrounded by a plexiglas blast shield, and placed immediately next to the bare BF_3 detector, which had 20" of polyethylene moderator directly behind it. In this way, we should have been sensitive to both thermal, and fast neutrons, if any came from stress-induced fusion as the Pd lattice unloaded[19,20]. No neutrons above backgrounds were seen for 40 minutes, and then the piece was removed from the liquid nitrogen allowed to warm up. No neutrons above background were seen in this phase either.

After removal from the liquid nitrogen, we rested the Pd stone on a piece of wood (in order to check for possible catalytic scalding, due to recombination[10]). The surface of the darkened piece, which quickly became frost covered (from humidity in the air) warmed to 0°C after 15 minutes. A thermocouple in close contact with the main Pd stone, saw no thermal excursions, during the next hour, and so we let it sit overnight on a piece of tissue paper. No evidence of catalytic recombination of deuterium at the surface of our piece of Pd was seen. It is interesting to note here, that Lewis[5] indicates on pg. 68 of his book, that "specimens with (originally) smooth bright surfaces generally have relatively poor catalytic activites"....but "the quantity of hydrogen which is finally absorbed in the steady-state may in fact substantially exceed the quantity of hydrogen which can be absorbed by electrodes with surfaces of a high activity..."

3.3.2 Mass increase

The Pd stone was weighed before and after the experiment. It clearly had devolved a significant amount of deuterium (as evidenced by vigorous bubbling after cell shut-off) during the one hour it was in liquid nitrogen, and the further night it sat exposed to the air, while warming up. Nevertheless, the mass uptake (24.0290 - 23.7759 gms) of 0.31 gms corresponds to a ratio of 0.70:1 D:Pd, if any possible lithium is neglected. This is close to values of 0.67 reported in the literature.

Weighing the Pd after the experiment is in general problematic. The piece must be dry, and yet at the same time, you don't want to let the deuterium unload significantly, so the measurement needs to be made quickly. From videos of the bubbles unloading from the Pd, we crudely estimate a loss of 0.3-1 liter of D_2 from the Pd, before it was weighed the next morning. Titrating the electrolyte puts a limit of the Lithium that was unaccounted for at 5-10% of the original amount added, or equivalently, less than 15 mg.

Subtracting the mass of Pd in the thin connecting lead, we are confident that the Pd stone itself was loaded in excess of 0.8 D:Pd, and most likely in the range of 0.9 to 1.2 D:Pd.

3.3.3 Post-mortem analysis

After a period of f ur ays, we cut the stone into two pieces, without much difficulty, using a hacksaw. The stone was hardened, and embrittled, relative to an unloaded piece of palladium. We etched the smaller piece with a weak solution of *aqua regia* acid in order to better see the crystal structure. After rinsing in distilled water, we placed it underwater to view the continuously outgassing bubbles. Figure 6 shows Polaroid images of the cut, and etched surface, at 40× magnification. Bubbles are easily visible,

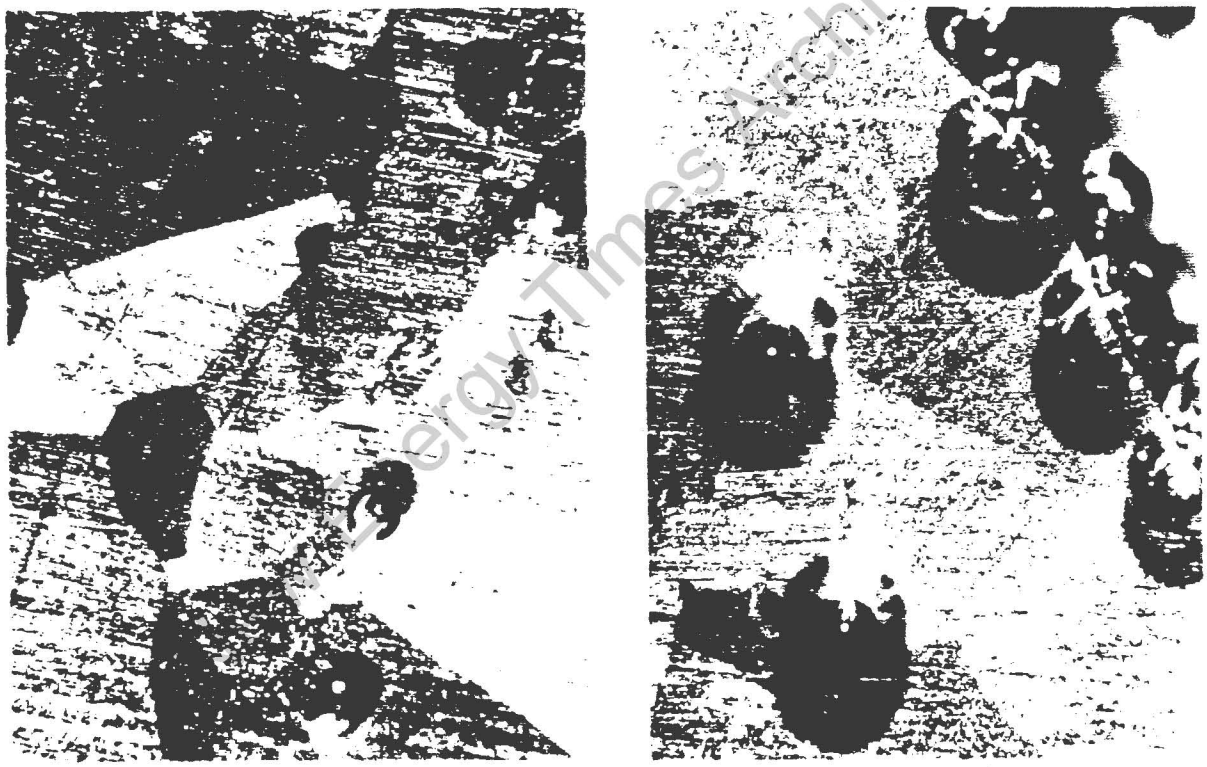

Figure 6: *Microphotograph of a slice of the Pd stone, showing the large crystal grain structure, and escaping gas bubbles, four days after termination of the experiment.*

and originate at the edges of the stone, and also along crystal grain boundaries. We were surprised at the large size (2mm×2mm) of the grains. We also could see evidence of small voids in the stone, evidently a relic of the original casting.

We noticed six weeks later, that significant fractures in the stone had developed, as the stress of the escaping deuterium fissured the piece, predominantly at crystal boundaries. At this time, electron microscope pictures were taken, showing the fractures occurring along crystal boundaries, as can be seen in Figure 7. Interestingly, three days after

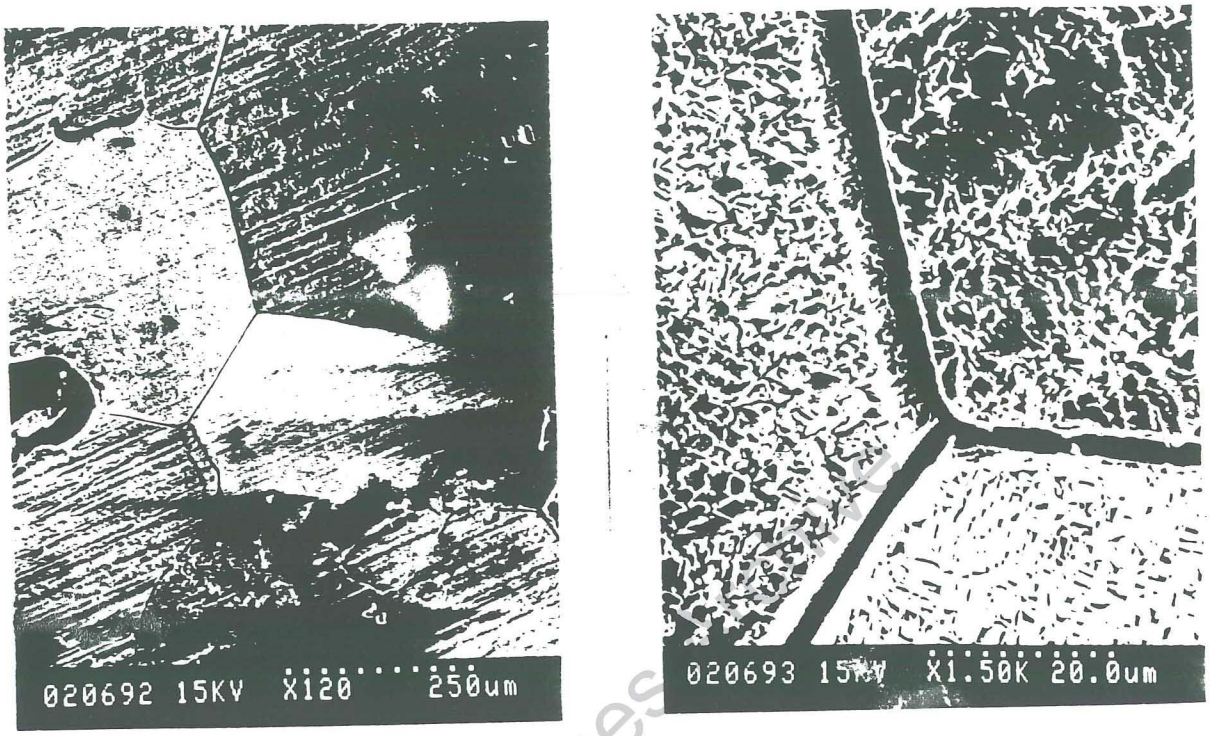

Figure 7: *Electron microscope photos at two magnifications, showing the large fissures that developed in the Pd stone, as deuterium escaped, weeks after loading.*

exposure to high vacuum, and the bombardment from a 15keV electron beam of the scanning electron microscope, we noticed that the Pd piece (stored in a capped glass vial) was visibly wet. It had finally become catalytically active, and was recombining escaping deuterium with oxygen in the air! Absorption photometer scans of 100 microliter samples of the liquid, showed it to be heavy water with an isotopic purity of 85%, with the normal hydrogen coming from water in the air. Scans of normal water, the catalytically regenerated water, and that of the "pure D_2O", are shown in Figure 8 for comparison with various known dilutions. The absorption bands of water between 3000 Å and 3.2 μm allow a quantitative determination of the isotopic ratio between light and heavy water. However, due to the 8-week (post experiment) intermediate exposure to air, immersion in normal water for previous bubble photographs, etc., this represents only a lower bound on the deuterium fraction in the freshly unloading Pd piece. Heavy water continues to be generated, ten weeks after the run was ended. Just as a check, similar scans of stored samples of the #3 electrolyte after the run, showed that its isotopic purity remains greater than 99% D_2O.

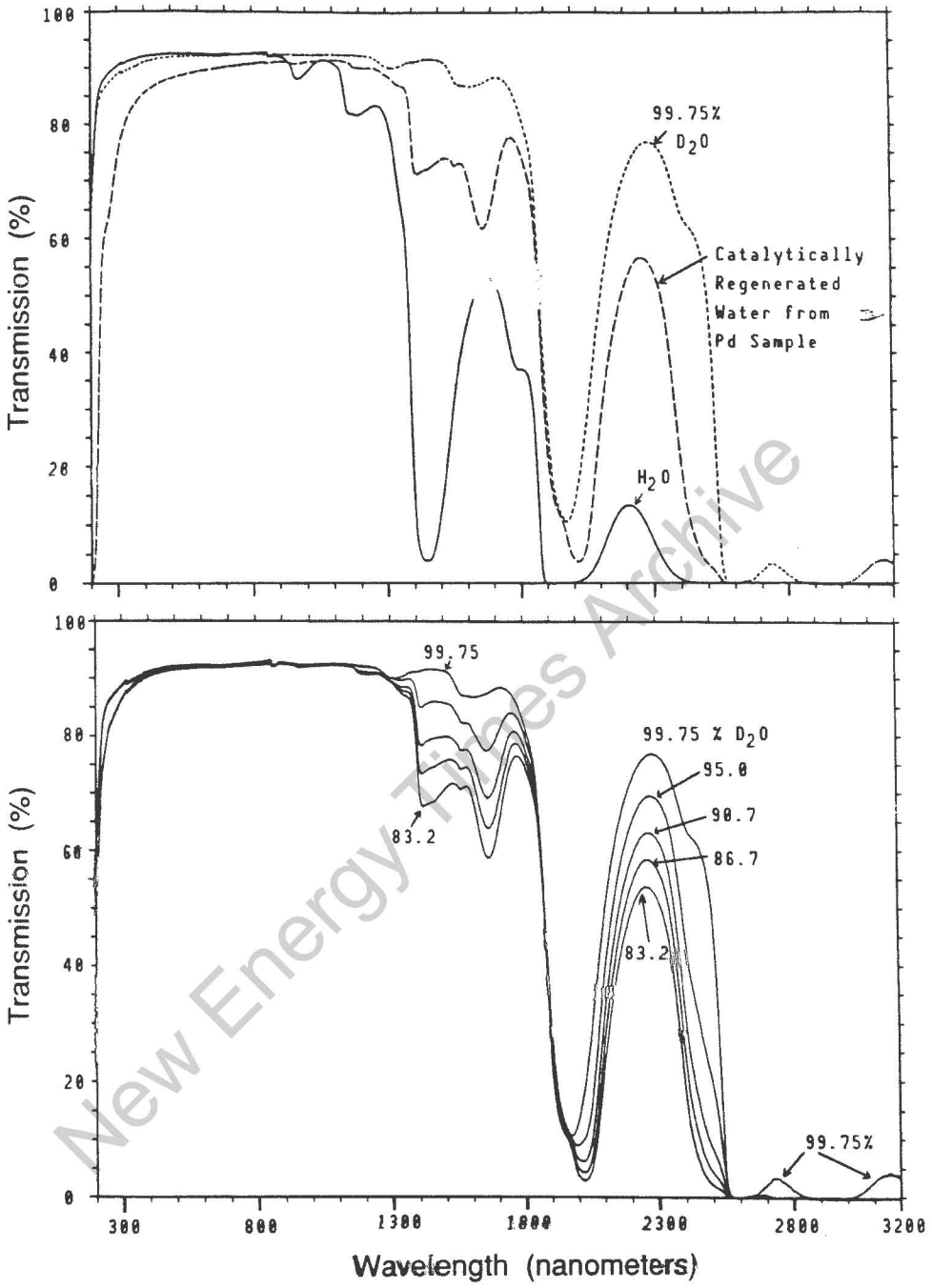

Figure 8: *(a) Absorption spectroscopy between 300 nm and 3200 nm, comparing the catalytically regenerated water from the surface of the Pd, with normal water, and 99.75% purity heavy water. (b) A series of dilutions of the high purity D_2O with known amounts of light water, enabling a quantitative determination of 85% deuterium concentration in the catalytically regenerated 100 μliter sample from the Pd.*

4 Neutron Diagnostics

We decided initially to look for thermalized neutrons, and not to attempt neutron spectroscopy until we first saw some neutrons. Therefore we used a sensitive BF_3 detector and an absolutely calibrated radiation monitor system (Berthold LB1026).

4.1 BF_3-detector

The BF_3 detector was a 2" diameter proportional counter with an active length of 31.1 cm, filled with 0.933 bar of BF_3 (Reuter-Stokes, Mod. Nr. RS-P1-1613-203, Ser. Nr. W-4452) enriched in ^{10}B. It was operated at a bias voltage of +2100 V with a Canberra 2006E preamplifier and a Canberra 2015A Amplifier/Timing Single Channel Analyser. The counting pulse from the Amp/TSCA was fed into an Canberra 1772 Counter/Timer (and onto a strip chart recorder), and the amplifier output was checked on a Pulse Height Analyzer Nuclear Data ND 66 with a Nuclear Data ADC ND750. The counter as well as the spectrum analyser were used to measure the neutrons, in short time intervals (about 10 min.) and in longer integration periods (up to 26 hours) respectively.

Figure 9 shows the BF_3 detector pulse height distribution, with markers indicating the region of interest corresponding to expected neutron pulse heights.

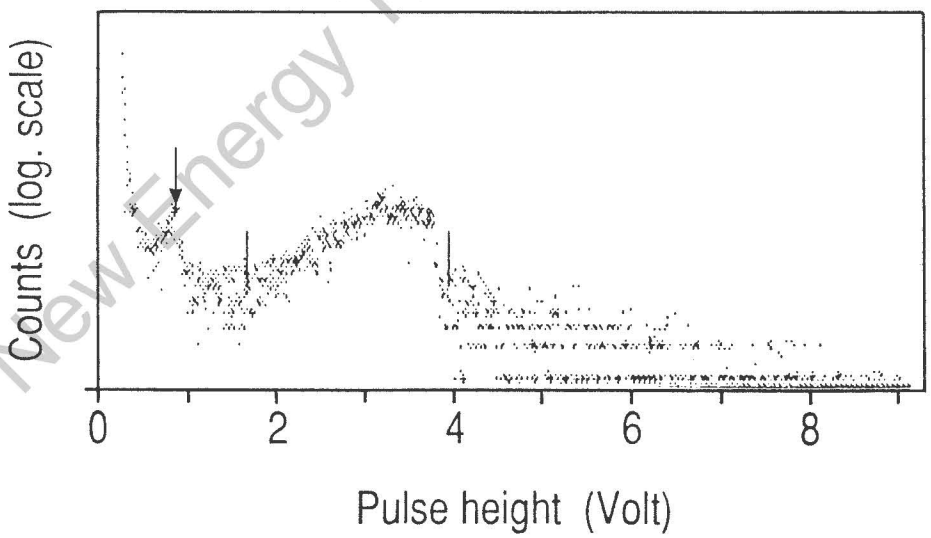

Figure 9: *Pulse Height Distribution of the BF_3 proportional counter (2" at 0.933 bar). Background measurement for 25 hours. The region of interest used as neutron signal is indicated with the markers.*

This, however, is not a spectrum of the neutrons, but one of the charged products which are created in the BF_3 gas with the reaction $n + {}^{10}B \rightarrow {}^{7}Li + \alpha + 2.792$ MeV [21], and gives no information about the actual neutron energies. The low energy peak (marked by the arrow) is due to pulses counted in the dead volume at the ends of the

detector [22], and its position is independent of bias voltage (unlike the main spectrum). The spectrum doesn't show the expected structure with two distinct peaks, but that might be due to wall effects and unoptimized electronics.

The detector was in a polyethylene moderator block 25 cm × 25 cm wide and 50 cm high. Not knowing the structure of the neutron background this moderator block was surrounded by 0.5 mm thick cadmium sheets to "block" thermal neutrons from background. This cadmium shielding, however, didn't change the background rate significantly. Before its installation we measured 3.81 ± 0.07 counts per minute (cpm) in the spectrum, and with the Cd shield 3.75 ± 0.13 cpm. A day before this installation the background rate was 3.33 ± 0.06 cpm, which simply indicates the variability of the cosmic ray background in time.

The lower and upper thresholds on the TSCA were set correspondingly to count roughly the same part of the spectrum.

After the first experiment we calibrated the BF_3 detector with a ^{238}PuB neutron source, absolutely calibrated in August 1977 as emitting 6.81×10^6 neutrons per second, therefore emitting 6.21×10^6 neutrons per second in April 89.

The source was positioned above the electrolytic cell, just in the same distance from the BF_3 tube as the Pd cathode (20 cm), but without any water inbetween. Actually there is about 3 cm of water between the palladium and the moderator which would moderate neutrons before entering the Cd sheet therefore giving smaller count rates. Thus our calibration factor (neutrons/count) will be a lower limit to the true value. With the strong source at this position the neutron counter showed 14992 ± 27 counts per second. From a similiar detector used for neutron flux measurements on ASDEX we know that the dead time is about 6 μs. This results in a dead time corrected countrate of 16.48 ± 0.27 kHz and a calibration factor of 377 ± 6 neutrons per count.

The pulse height analyzer had more problems with the high count rates for its particular calibration. For a real time of 60 seconds, the ADC had a live time of only 39 seconds and the spectrum showed strong pile-up. When we also used the pile-up peak we got 7.4×10^5 counts, corresponding to 327 neutrons per count. Since it is not clear how to take the pile-up into account correctly we attached a large uncertainty of at least 20 % to this calibration factor, and used only the separate counter as a calibrated diagnostic.

4.2 Neutron results

Figure 10 shows the neutron measurement (from the counter) during experiment #3. The counter data were not recorded automatically, but only manually, and therefore are not continuously available.

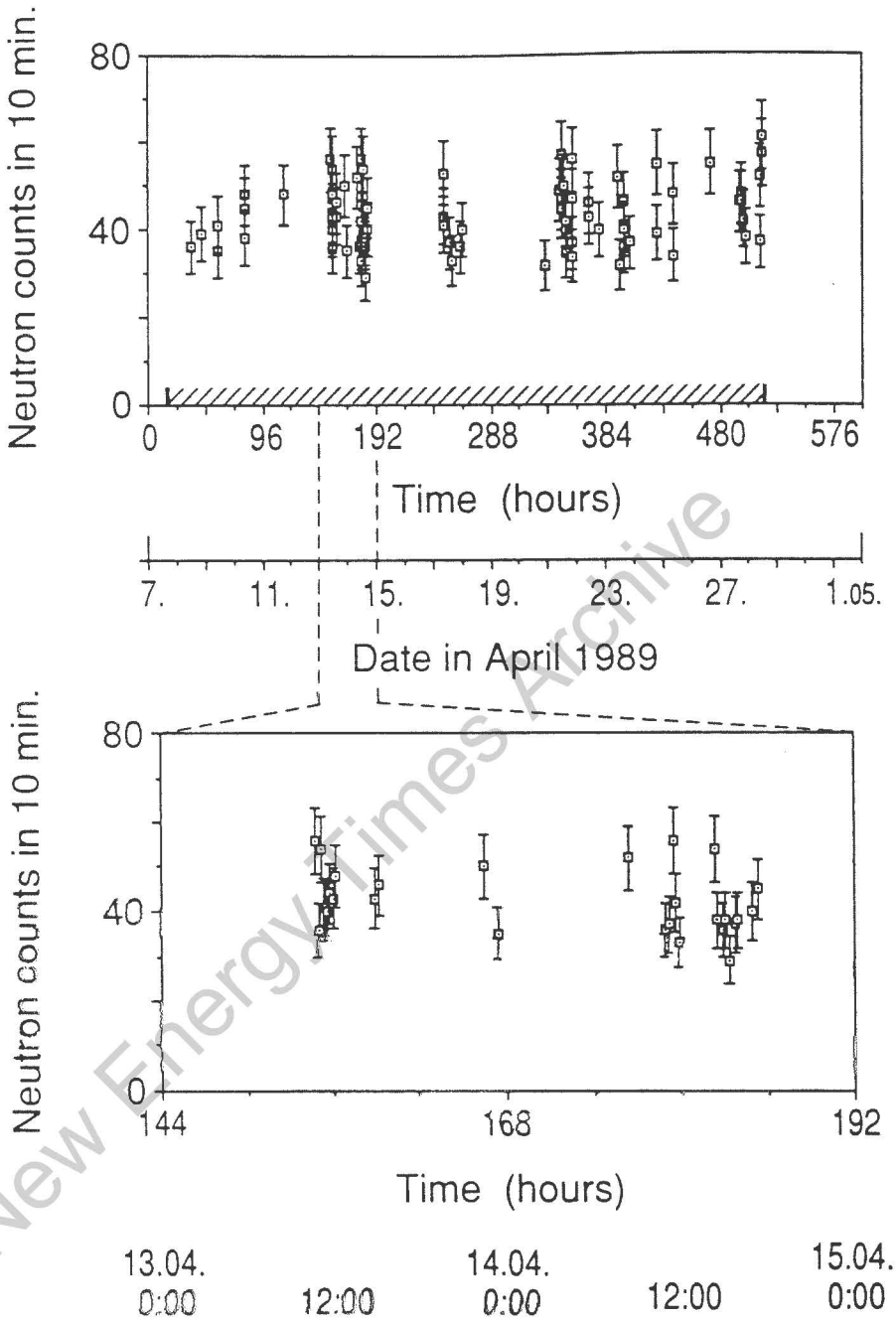

Figure 10: *Neutron background rate during experiment #3. The time scale starts at April 7 00:00 with 0 hours. The data are taken in 10 minute intervals. The average count rate of 4.2 ± 0.7 counts per minute corresponds to a neutron source rate (if from the test cell) of 26 ± 4 neutrons per second. The lower part of the curve shows a 2 day period with a larger time scale to demonstrate the background fluctuations. Cross hatch denotes duration of #3 cell operation.*

The experiment #3 was started April 7 at 17:12 which is 17.2 hours on the time-axis and it was stopped April 28 at 15:50 (519 hours). Large fluctuations are seen in the neutron background counting statistics, but there is no systematic change in the count rate observable over the course of the experiment.

Figure 11 shows neutron levels from evaluation of the BF_3 pulse height spectra. Although we do not have an absolute calibration for this figure, it is quite obvious that

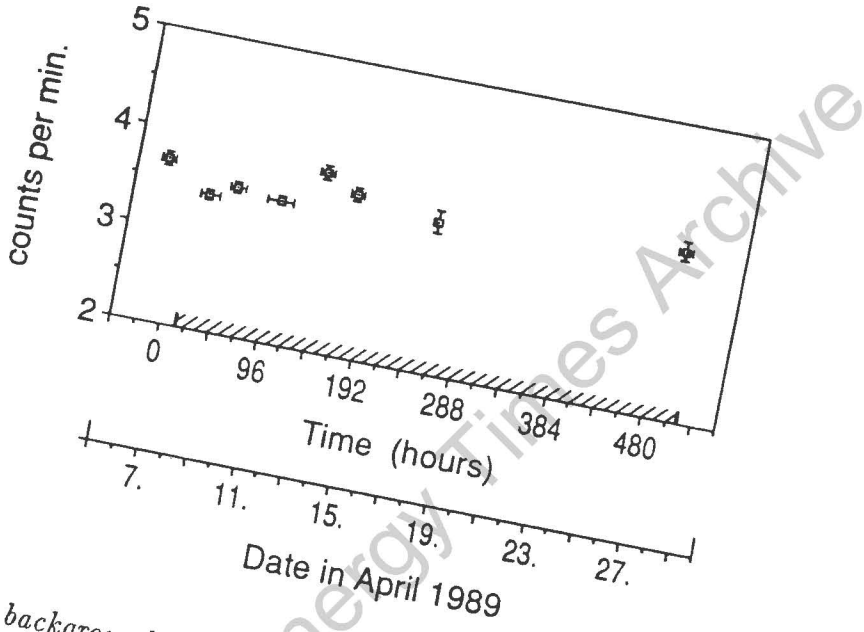

Figure 11: *Neutron background evaluated from the BF_3-spectra. The sampling durations vary between 4 and 26 hours, indicated by the error bars.*

due to the long sampling times of the spectra the background fluctuations are reduced. The average rate during the experiment #3 is 3.6 ± 0.15 cpm which is a standard deviation of only 4.2 % instead of 16.6 % for the 10 minute counter measurements. The spectrum taken before experiment #3 has a mean count rate of 3.75 cpm thereby showing hat the neutron rate during the experiment did not only show no change over time but lso its absolute value was not larger than before switching on the electrolytic cell.

In summary, if we use our calibration factor for the BF_3 neutron counter (as if these utrons would come from the test cell) its count rate corresponds to a neutron source e of 26 ± 2.3 neutrons per second, for a 30-minute counting interval. A neutron signal a level of 3 σ above background, would correspond to 7 neutrons per second. We efore can conclude that the neutron rate from our electrolytic cell was smaller than eutrons per second per gram of palladium, or equivalently, $< 1 \times 10^{-22}$ (ddn) fu-/dd pair/sec.

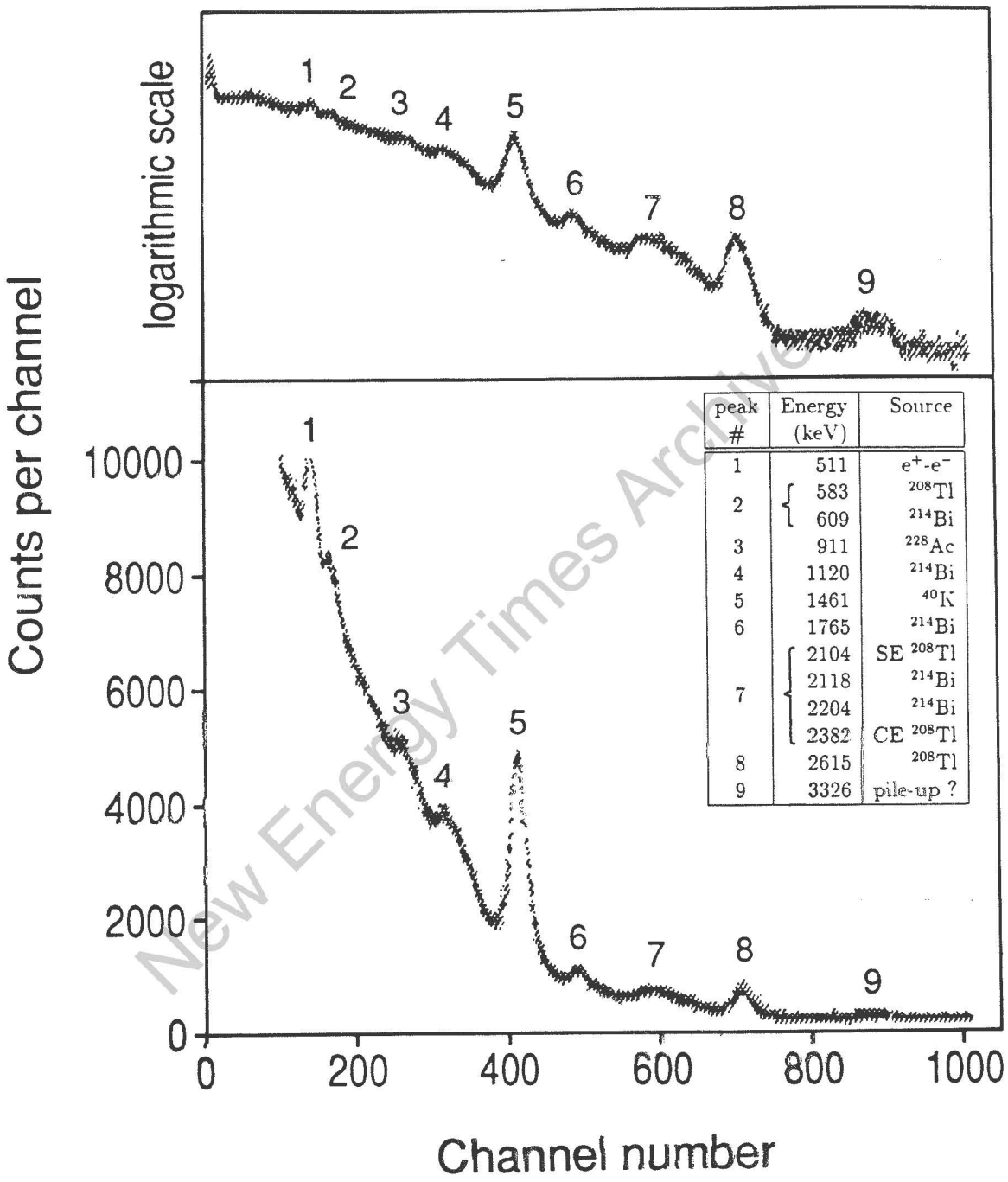

Figure 12: *γ-background spectrum measured with a 2" × 2" NaI-scintillator for 96.4 hrs after #3 (May 4–8). Identification table is made by using a high resolution Ge(Li) detector.*

troscopy amplifier. The energy calibration was performed with uranium salts available in the chemistry lab.

In Fig. 13a the measurements of the γ-background with the NaI- and the Ge(Li)-detector are compared. The line at 2104 keV is the single escape peak of the ^{208}Tl line at 2615 keV. This shows dramatically that the spectrum measured with the NaI detector is dominated by unresolved background lines, easily resolved with the higher resolution (2 keV instead of 90 keV at 1.46 MeV) Ge(Li) detector.

Fig. 13b shows spectra from a ^{226}RaBe neutron source. Since ^{226}Ra is a member of the uranium decay chain, this source also contains ^{214}Bi. The single escape peak of the ^{208}Tl line is not visible since this spectrum has been collected for only 30 minutes. The lower curve shows the pure γ-spectrum of the source, while for the upper one a 9 cm thick water tank had been put between source and detector to generate the 2224 keV line from neutron capture in hydrogen. This figure shows clearly that this weak line is too close to the Bi-lines to ever hope to separate it with an NaI-detector.

If one wanted to measure the neutron capture line on top of the broad Bi-spectrum, one would have to reduce this background (namely the concrete surrounding the experiment), but even then the sensitivity (for detecting neutrons) would be poor in comparision to standard neutron diagnostics.

6 Radiation Safety Monitoring System

In addition to the above described detectors, we used an absolutely calibrated radiation monitoring system from Berthold (LB1026-2) which measures separately the neutron and the γ-dose. It prints out every 24 hours, if ever a pre-set threshhold was exceeded (this was useful if anything happened while we weren't in the laboratory!).

Its neutron detector is a ^{6}Li-I detector in a Bonner sphere, and was located in an adjacent fume hood, about 1.5 m from our electrolysis cell. Gammas were measured by a wire proportional counter, placed directly next to the electrolysis cell, about 15 cm away from the Pd piece.

Figure 14 shows the neutron dose (upper curve) as well as the γ-dose (lower curve) in μSv per day. Both radiation doses show no significant change over the period the experiment #3 was operated (April 7 to April 28), exept on April 26 and 27. On those two days we used a 2 mCi ^{226}RaBe source in the lab to calibrate our other detectors. The γ-dose for these days was 99 and 161 μSv respectively.

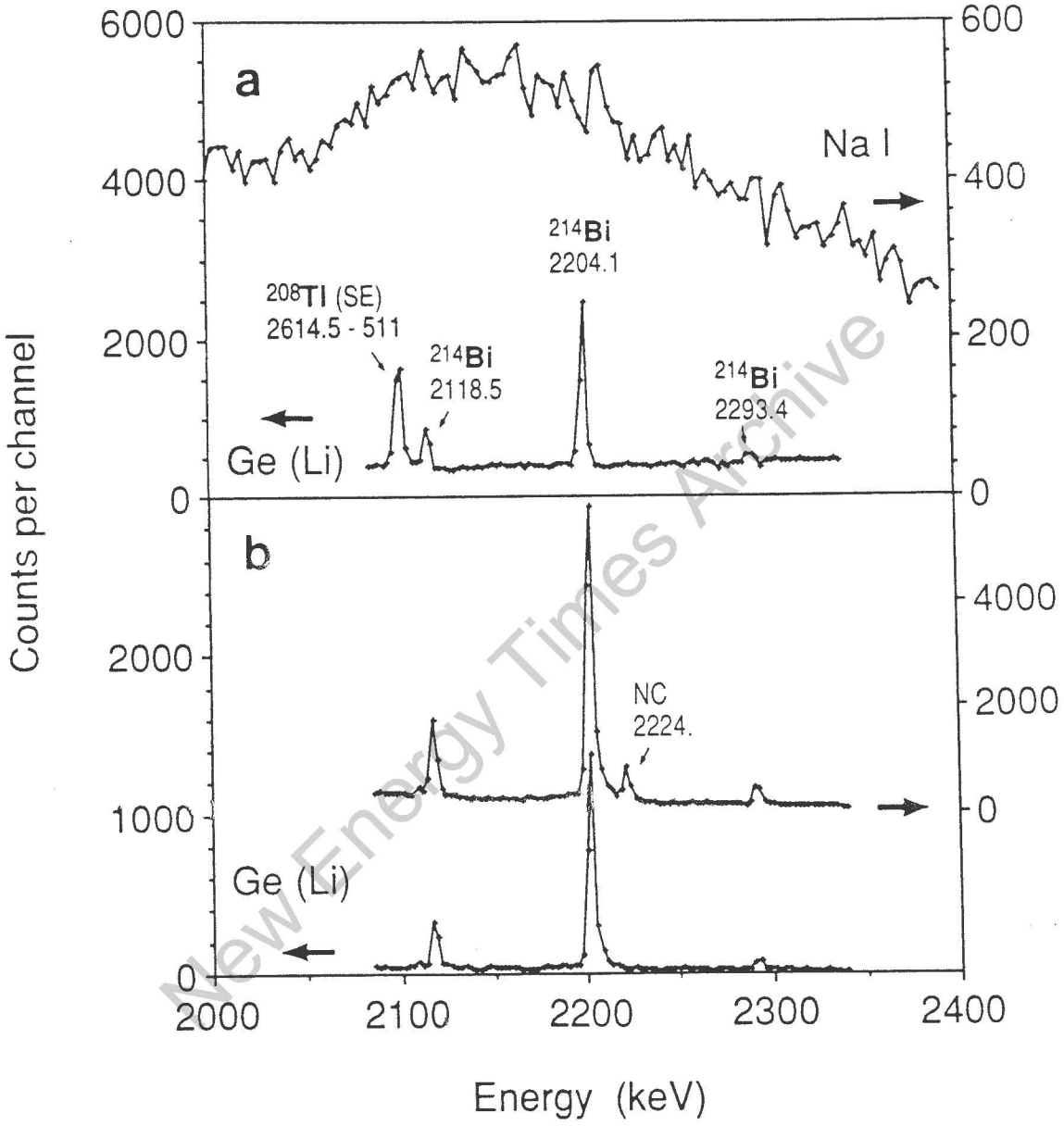

Figure 13: **a)** *Comparison of the γ-background spectrum from 2.0 - 2.4 MeV measured with a NaI-scintillator (upper curve) and with a Ge(Li)-detector (lower curve).*
b) *γ spectra of a 2 mCi ^{226}RaBe neutron source, collected for 30 minutes. The lower curve shows the pure γ-spectrum of the source, while for the upper one a 9 cm thick water tank had been put between source and detector to generate the 2224 keV line from neutron capture in Hydrogen.*

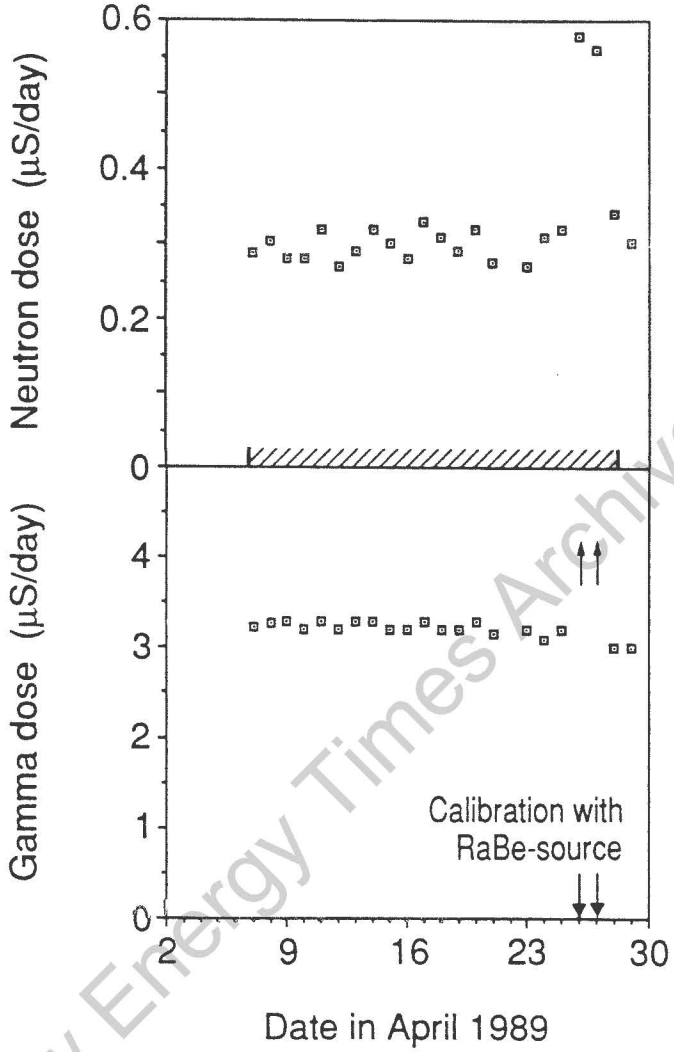

Figure 14: *Absolutely measured radiation doses in the chemistry lab. The electrolysis experiment #3 was operated from April 7 to April 28 and there is no significant change during this time. There was, however, an increased radiation level on April 26 and 27 when we used a 2 mCi $^{226}RaBe$ source to calibrate our other detectors.*

7 Calorimetry Measurements

Calorimetry measurements form the heart of Fleischmann and Pon's experimental claims. Without the measurements of "excess heat" which are claimed to be nuclear, and not chemical in origin, the entire world would never have paid attention to their press conference[3]. Truly careful microcalorimetry was beyond the level of sophistication of our i al experiments. Nevertheless, we recognized we could do an initial job at calorimetry h a large piece of palladium, detecting heat at levels above 100 mW, to easily see amounts claimed from the larger Pd pieces reported at Utah. In particular, we wanted to verify any correlated neutron activity (should we actually see any neutrons). We also knew that a careful accounting of our input powers would be necessary to determine if in fact we actually had "excess heat", and to differentiate against chemical sources, if the need arose.

However, our calorimetry effort from the start was a second order effort, and we believe only marginally better than that of F&P. In particular, we had continuous temperature readings and good mixing at the location (in the water bath) where our temperature measurements were made. But like F&P, our cell was not closed or divided, nor was the energy content and identity of the escaping gases monitored, either which is required for accurate accounting of the energy balance.

7.1 Current-voltage behaviour

Current levels were largely determined by deciding what was an acceptable stable operating temperature...we *did not* want to evaporate large amounts of electrolyte, in our rather "open" first system. We used a current stabilized DC power supply in each experiment. Whenever the current was increased, the voltage would, after first increasing, then fall to a new (higher) level, over a period of time. This effect is at least partly due to increased mobility of the charge carriers in the solution as the temperature rises. Conversely, simple addition of cold water to the water bath was observed to transiently cause an increase in the cell voltage, for the same current. In a similar fashion, refuelling the center cell with 5 to 10 ml of D_2O at a time, would temporarily raise the cell voltage.

Determining the input power into the cell, requires first monitoring the I-V characteristics of the cell. The nonlinear, equilibrium, I-V curve for our large Pd stone (#3) experiment, is shown in Figure 15. We plot the measured differential voltage between the palladium and platinum leads, and the total current. Note that the cell itself *is* in fact a small battery, with a cell voltage (measured at 1 mA current) of 2.0 volts. We will later take the assumed Joule heating to be $I*(U-1.54V)$, as also *assumed* by F&P, where the 1.54 volts represents work performed by electrolytic dissociation of the water, as well as other losses due to surface bubble conditions, and contact potentials. In truth, the assumed constancy of the 1.54 volts *should* be directly measured[24], but we *did not* do it in our experiments. This could be a source of error, especially when making comparisons to different equilibrium currents (and hence rates of bubbling).

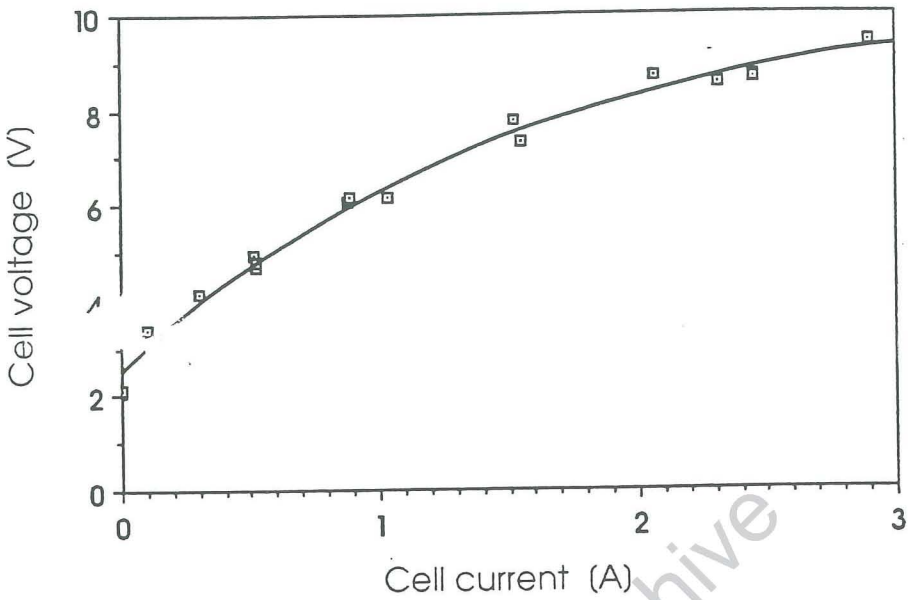

Figure 15: *Equilibrium cell impedance is a function of the electrolyte temperature, bubbling rates, and the applied power. U-I curve for the #3 cell shows the characteristic decrease in resistivity for higher cell currents (temperatures), as the ion mobility increases.*

7.2 Temperature differentials

The principal results of our calorimetry efforts, other than generating rolls of straight lines on the multipen strip chart recorder, are shown in two parts. Data from before (Figure 16a) and after modifications to the waterbath, (Fig. 16b) taken during the long duration #3 experiment, are plotted against the calculated Joule input power to the cell. In both cases, we plot the temperature rise of the approximately 750 ml stirred water bath, over and above the ambient air temperature of constant flow cooling air being sucked into the fume hood, versus the assumed Joule heating.

The first plot in Figure 16a shows a substantial offset temperature for low currents. This effect turned out to be due to a second source of heat to the cell, coming from the motor of the magnetic stirrer used to stir the water bath! It represents the effects of 1.6 W of heating power. The palladium in this experiment has a volume of 1.8 cm^3, so the reader gets a good comparison to the much larger expected magnitude of the reported 20 W/cm^3 excess heating of F&P, should it be present! When the stirrer was turned off, this temperature differential went to zero, as suspected.

Consequently, we improved the water bath geometry (with the cell continuing to run!) at 315 hours into the experiment, by adding insulation and improving the waterbath circulation. A schematic of the cell, and issues affecting heat flow is shown in Figure 17. We found by accident, (as one should in retrospect, expect), that simply leaving the hood in an "up" position, decreased the cooling by an equivalent of 1.7 watts. Consequently

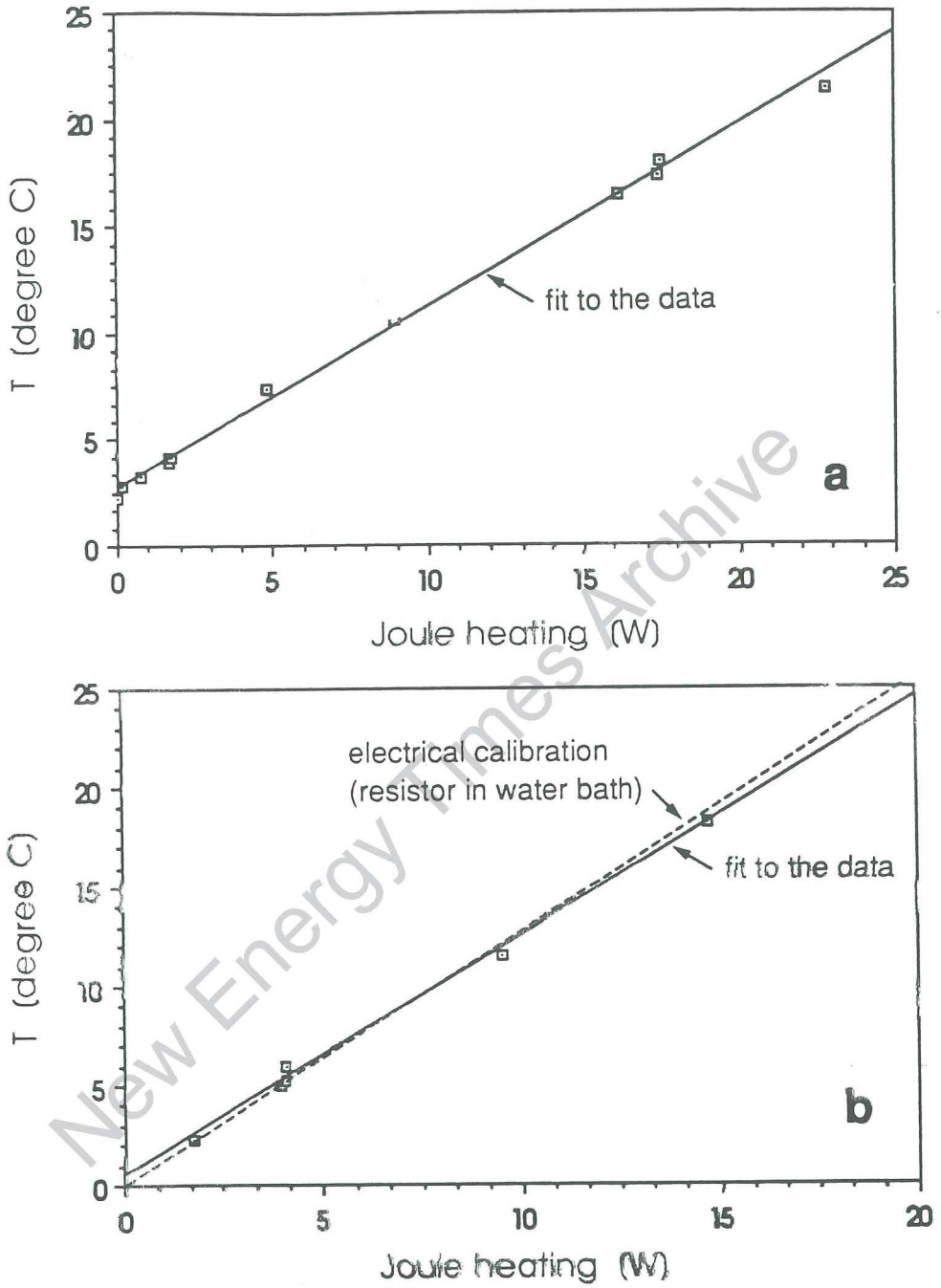

Figure 16: *(a) Temperature differential of water bath relative to cooling air, as a function of cell Joule heating, obtained by scanning the cell current, but always waiting at least three hours for new equibrium in the water bath.(b)Temperature response in #3 palladium stone experiment, after bath modifications. A strip heater resistor in the waterbath allows direct electrical equivalent comparisons, while the cell is in operation.*

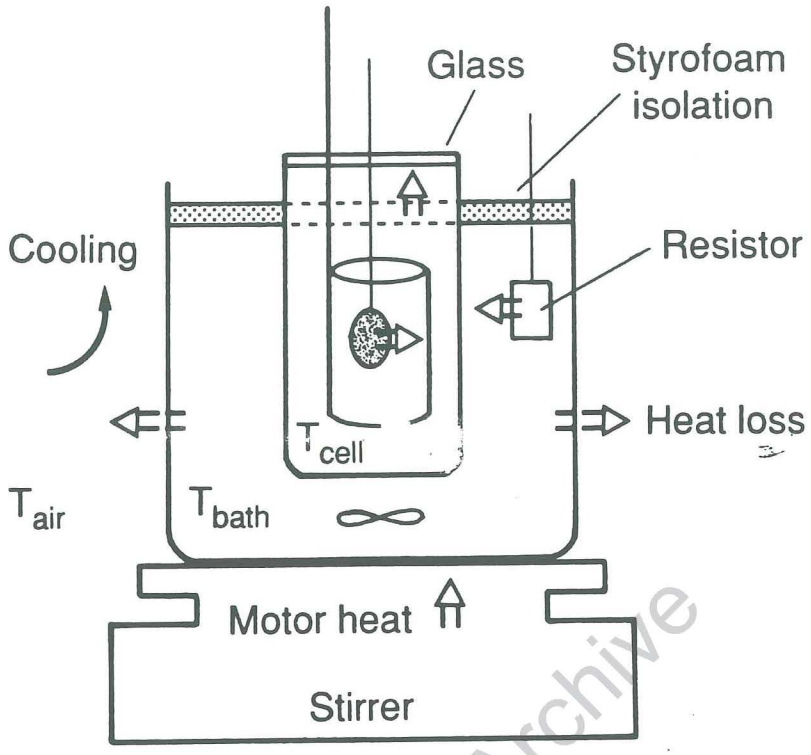

Figure 17: *Schematic of final configuration for heat flow in the #3 experiment.*

we were careful to make measurements in our so-called "hood down" position, which also gave us a more well-defined air flow. Effects from the stirrer motor were reduced to 300 mW power, absolute. The new temperature vs. power line is shown in Figure 16b, including also a strictly Ohmic heating calibration set taken by running a small current through a Kapton encapsulated 40 Ohm resistor placed in the well-stirred water bath. These calibration points were taken simultaneously with the cell operating, at three different equilibrium waterbath temperatures, between 29 and 49 °C. We do not fully understand why the electrical calibration has a larger slope than the curve produced by running the cell at different currents (temperatures). We may be seeing the effects of enhanced evaporative losses from the cell, as less of the cell heat is efficiently transferred to the water bath, and a larger fraction is lost in the saturated vapors escaping from the cell at higher temperatures and/or bubbling rates. This effect was in fact mentioned as a possible problem by A. E. Pontau from Sandia National Labs, at the Santa Fe Workshop, May 24, 1989.

In summary, our calorimetry, which in and of itself is filled with difficulties, nevertheless would have allowed us to easily observe effects an order of magnitude smaller than reported by F&P. At the 5% level, we saw no excess heating. Furthermore, we looked for transient effects resulting from rapid decreases in cell current, and after waiting for our waterbath equilibrium (In the #3 setup, time constant $\sim$ 100 minutes), could not see any unusual effects. In our thin sheet experiment (#2), thermal conditions were in equilibrium after four hours, and remained identical for the remaining forty odd hours of the experiment. Fast transients due to loading (or unloading) were most easily seen in the

first thin-walled tube experiment (#1), due to the short three-minute cell thermal time constant in the center cell itself, but it was precisely these types of measurements which made us appreciate the difficulties with large thermal gradients in the "gas sparged" center cell.

8 Tritium Measurements

As one part of their "proof" of nuclear reactions occurring in their electrochemical experiment, Fleischmann and Pons offered the measurement of tritium in the electrolyte as a sure sign of the fusion of deuterium. Unfortunately their paper[1] *does not* describe the levels of tritium found in the D_2O used before the experiment even began!

What we found, as have others [10], is that the tritium in the D_2O differs from bottle to bottle by as much as two orders of magnitude, as supplied by Merck, irregardless of the effects of further concentration/separation by several weeks of electrolysis. The tritium level (100 decays/minute/milliliter) found by F&P easily lies in the middle of the range we detected in our 99.75% D_2O, Merck Article # 2919 UVASOL flasks.

We used a model 2260XL Tri-Carb Liquid Scintilation Analyzer by Canberra-Packard to measure 6 ml samples of our stock D_2O flasks, samples with LiD (pH $\sim$ 12, unneutralized before making up the cocktail gel) in both D_2O and H_2O, and the actual electrolyte after the first, and during and after the third runs. An efficiency factor of 30% was measured from a tritium standard, and used to convert counts/minute (cpm) into decays/minute (dpm). The measurements were kindly performed on the new PS/2 controlled analyzer, in the Nuclear Chemistry group of the Reactor at the Technical University. Spectra from the instrument indicated that chemical fluorescence was not a significant problem, and had no bearing on the measurements. The first measurements were performed 19 days into the third (and final) palladium experiment. We had also saved samples from earlier runs, and also the stock D_2O bottles. Unfortunately, prior to this, we had not located a suitable tritium measurement instrument that was accessible for our purposes.

We learned from these measurements, that premixing of *all* D_2O to be used in a run, would be absolutely essential for an accurate determination of any alterations in the prexisting tritium levels. However, we did not do that, and also initially didn't have a feeling for how much (expensive) heavy water we would be "using" up over the course of the experiment. Consequently, as we used solutions made up from three 100 ml Merck flasks (one of which was 100% used up before a tritium measurement was made), and one 250 ml Merck flask, we are unable to precisely calculate the various dilutions which occurred.

The results of the tritium measurements are shown in Table 4.

Table 4: Tritium levels in IPP experiments

Sample #	(dpm/ml)	Description
1	6.6	H_2O+LiD
2	9.9	99.75% D_2O 250ml Merck flask
3	210	99.75% D_2O 100ml Merck flask
4	750	D_2O+LiD After #1 Exp. (14 hours)
5	150	D_2O+LiD During #3 Exp. (19 days)

They indicate, that levels ranging from 10 dpm/ml to 750 dpm/ml, were found in the bottles of high purity heavy water from Merck, even though they were bought at the same time (about 15 years ago), and have the same label numbers on the bottles! We also worried about tritium being added through the lithium deuteride that we used as the "salt", but this was measured by testing a blank mix of LiD in H_2O, at 7 dpm/ml, and therefore was not a problem. Another concern was that one of our hot neutron calibrations could have actually bred some tritium in the lithium. A quick worst case estimate from the 12 Curie Plutonium source used in this example, showed this would have only a trival effect from the less than hour long exposure. Due to heavy water replenishment of the Pd Stone (#3) experiment mostly from the 4^{th} and largest bottle of D_2O, which also had a low tritium level, the final solution in the third experiment had a lower concentration of tritium when it ended, than when it began.

We conclude that all of the tritium levels which we saw in our experiments, could be accounted for, simply, from the tritium initially present in our heavy water. While changes in the concentration undoubtedly occur due to distillation and separation by the Pd metal as it loads, we don't expect more than factors of 3-9x here[25]. Furthermore, our results span those of Fleischmann and Pons (from a factor of 10 lower, to a factor of 10 higher), and make us concerned whether F&P also did not premix their heavy water (since the variability of tritium in heavy water is so high), as well as what the absolute levels of the tritium in their heavy water actually were before they began their published experiments. As an erratum[1], F&P have finally stated that their tritium level went from 41 dpm/ml to 141 dpm/ml, in their electrolyte. This is well within the range of expected isotopic separation effects!

Some critics of F& P have indicated that chemical fluorescence (due to poor neutralization of the strong base, and subsequent chemical reactions in the "cocktail gel") accounts for spurious tritium measurements. We do not support this point of view in our equipment, and indeed, did not even need to neutralize the sample electrolyte at all prior to mixing the "cocktail". Fluorescence was accounted for in our measurements, and made an effect only at the 5% level.

9 Conclusions

9.1 Our experiments

We have performed integrated experiments in three electrolytic cells; two test experiments and one "final" long run-time (for 3 weeks) setup. During this experiment the cathode was a 22 gram vacuum-cast "stone" of palladium. After the run the P[illegible] was loaded with D_2 in excess of 0.8 D:Pd, probably even in the range of 0.9 to 1.2, as determined by mass uptake. Absorption spectroscopy of catalytically regenerated water from the outgassing Pd sample weeks after the electrolytic loading, confirmed the presence of large amounts of deuterium. Forced, rapid changes in cell current, after long periods of equilibrium, yielded nothing unusual.

During the runs we didn't observe any neutron emission above the background, which for our setup means an upper limit on the neutron emission from the electrolysis cell of 1×10^{-22} fusion reactions/dd pair/second. The gammas from neutron capture were also monitored, first with a NaI-scintillator, later with a Ge(Li)-detector which is able to separate the neutron capture gammas from the natural background, but we did not see a signal attributable to the cell there either.

Our calorimetry was simple, but trustworthy. We should have been able to see excess heat production an order of magnitude smaller than reported by F&P, if present, but *did not.*

Our measurements of the tritium content in heavy water demonstrated that there are large variations between different source bottles, even those bought at the same time and from the same vendor. The tritium content measured in our experiments can easily be explained in this fashion, or with well-known isotopic separation effects.

We did not attempt to measure helium isotopes in our palladium samples, although we discussed the need for extremely high resolution mass spectrometry to resolve among the various mass combinations of H, D, and T, against possible He isotopes.

By virtue of our quick start in this affair, we recognized at an early date (in comparison to others who came later with sometimes more precise measurements), that extensive and expensive efforts would be fruitless. It is our opinion, based on our own experiments, other papers cited here, and visits to other laboratories involved, that no fusion processes are required to explain the results of Fleischmann and Pons.

9.2 Probable errors in the F&P experiment

After 4 weeks of experiments, the three described above, and additional mock-up normal water cells, as well as a heavy water cell used for fast loading/unloading experiments, we have concluded that the work of Fleischmann and Pons was unfortunately hastely written and not up to normal scientific standards. The lack of controls was particularly conspicuous.

We have identified weak points in our own experiments, and based on what we know

of the F&P experiment, likely errors in the F&P experiment. We have also pointed out differences, and open questions.

- The thinking and design of the F&P calorimetry is not carefully laid out in their publication[1]. Supposedly it was to follow in their larger, more detailed article (later withdrawn from Nature[26]). Our own calorimetry was easily capable of seeing the large (~30%) effects claimed by F&P. We did not see any such effects in our experiments. Issues of evaporative losses (dependent on the operating temperature), unknown products being lost in the open system (through the exhaust gases), variable effects of bubbling at different current levels, uncontrolled catalytic recombination, all remain for researchers with finer calorimetric equipment than ours.

- Arguments by Pons at the time of the Los Angeles ACS meeting, and his showing of a video that a tracer dye was well-mixed in their tube after 20 seconds, by action of the bubbling, *in no way* counters the fact that stable convective cells would allow the existence of significant thermal gradients in the tube, while still having eventual mechanical mixing, so long as the heat source is generated at one place in the tube (as was the case).

- Our doubts about the F&P gamma and neutron measurements, and their internal inconsistencies, have now been dealt with by a number of papers, and in particular, fully confirmed by the work of Petrasso, et al[27]. In a so-called "rebuttal" of the Petrasso work, by Fleischmann, Pons, Hawkins, and R. J. Hoffman[28], in *Nature*, F&P have only shown[29] that they never did a simple energy calibration of their gamma spectrum, nor looked up such a spectrum in numerous books on the topic. They have demonstrated a complete lack of credibility by shifting the energy of their so-called signal line by +300 keV (without explanation), and at the same time showing background lines where none are known to exist!!

- While we do not doubt that F&P indeed measured tritium at the 100 dpm/ml level, it is not clear that this tritium was actually *produced* in their experiments. Instead, we believe it to have been originally present in their feed D_2O, and weakly enhanced by well-known isotopic separation processes.

- The Pd/Pt/D/Li system can make a good "battery". Any discussion of *bursts* of excess energy from these cells, had better have extremely careful accounting of all energy balances, in order to discriminate against chemical origins.

  Based on the suprisingly careless work that has been so-far presented from this University of Utah group, we halted our own efforts in this endevour (in spite of having further cells "ready to go"). We have not seen any "believable proof" from F&P that the excess heating is nuclear in origin. This ideally means time-dependent data showing the relationship between the supposed "excess heating" and any nuclear products.

We thank everyone who helped without asking for an account number, and all the people who hoped it could be true. In particular: the initial gamma calibrations by Dr. Guenter Janeschitz using the ASDEX spectroscopy sources; the late night checks by Herr Spitzer; the cell manufacturing and lead shield machining in the ASDEX shop; the friendly loan of an expensive high efficiency Ge(Li) detector by Mr. Thies in the Technical University of Munich (TUM for short) accelerator group (thanks to Frau Dr. Schneider's connections); the tritium measurements by Frau Dr. Kim in the reactor group; the loan of calorimetry aids and advice by electrochemist Prof. F. Mayinger at the TUM; the raster electron microscope scans and x-ray impurity analysis of a piece of our #2 cell palladium by the Applied Electrochemistry Group of Prof. Dr. D. W. Wabner (also at the TUM); the scanning electron microscope photos of the Pd stone by the Max Planck Institute of Quantum Optics (Garching); and finally help from many interested on-lookers and kibitzers.

A Faxes, rumors, and papers: When, What and How we knew

In the fast-breaking world of press reports, computer networks, and publication by FAX, staying on top of the scanty developments following the initial F&P news conference, was a crucial part of our initial efforts. This effect was magnified by the paucity of details coming from the two Utah principals.

March 24: Some of us see an American video clip on Bavarian (Bayerische Rundfunk) TV news.

March 25: Short news clip on Armed Forces Radio.

March 26: Telephone calls from/to US. One of us (GAW) received a phone call from the USA, from his brother stating "Ok, I have my palladium, platinum, and heavy water....so what do I do next to make fusion?"

March 27: Computer network mail messages sent to associates in US, asking if they know anything about "this cold fusion stuff". Still an Easter Holiday in Germany.

March 28: First newspaper articles with some details. Our first library search turns up a wonderful handbook[4], 430 pages thick, entitled "Palladium", published by Verlag Chemie, edited by the Deutsche Chemische Gesellschaft in 1942, which as we continue to discover, is full of information useful in the coming weeks. We also obtain articles on various calorimeter configurations typically used by physical chemists.

March 29: We didn't fully realize why the cell was tightly closed, and had arguments about whether the cell might be pressurized (in order to aid the $\alpha - \beta$ crystal phase transition without boiling away the heavy water at temperatures of 150°C). We then realized that the electrochemical pressure itself easily drives this phase transition in the palladium cathode, and also that a pressurized glass vessel was unlikely for safety reasons.

March 30: Our first cell is switched on. We make a guess to use LiD as our salt. Low grade (95%) heavy water is used in our first trial.

March 31: Background measurements, following the shutdown of our #1 experimental cell. Fleischmann gives seminar at CERN.

An improved cell is "in the works". During the weekend of April 1-2, 1989, we continued to make background measurements, and collect summaries of news accounts, computer bulletin board rumors & facts, phone discussions with scientists in the US and England, generally trying to glean new information about the F&P experiments. Fleischmann had been at Harwell and at CERN, so a few more details were apparent.

April 2: Still no Fax copy of the rumored F&P paper, or the "other Utah" experiment of Jones... whom we knew to have been involved in previous muonic fusion measurements at Los Alamos, although some pretty good summaries of the contents of the papers were on the computer network.

April 3: We have a copy of the Jones preprint, but have no desire to use the so-called "Jones witches brew" of salts in the cell. Phone discussions with D. Robinson at Harwell.

April 4: Our #2 cell goes into operation, after our first receipt of the F&P paper on Monday evening April 3. Our copy came from Princeton Plasma Physics Lab, via an exchange scientist's suitcase (hence it suffered one less FAXing than many others here in Europe)! After reading it, we could confirm that the computer network news of the previous three days, which contained summaries of the paper, were reasonably accurate.

April 7: Our #3 cell goes into operation.

April 11: Newspaper accounts of Texas A&M claiming excess heat production. First rumors of "non-reproducibility" begin to surface from Harwell's discussions with Fleischmann.

April 14: We hear that it is essential to have a cast Pd piece, not drawn or extruded. By luck, this is exactly what we are running. Georgia Tech experimenters withdraw their erroneous neutron claims.

April 17: Pons press conference, claiming He^4 detection at U Utah... We obtain the Paneth & Peters 1926 paper. Our amazement that someone at Los Alamos had dug up an old German paper, was caused by a request from the editor of Nuclear Fusion to get a good copy of it out of our library. This short-lived report was on the transformation of hydrogen to helium in 1926, *with palladium*. It still causes us to wonder (in truth) whether if F&P ever knew about it, in relation to their own studies...

April 18: We obtain the retraction of the 1926 paper, and information from Sweden concering Prof. Tandberg, of the ElectroLux Laboratory, and his "fusion" experiments in 1928-1930 with electrolytic-loading of palladium in heavy water.

April 19: Pons gives seminar at Los Alamos. Los Alamos wants to have a collaboration to verify the "active" Utah cells, either by hauling cells to LANL, or sending people with measuring equipment to U of Utah.

April 28: We receive the palladium that we ordered at the beginning of our experiments. On the same day, we shut off our long-running #3 experiment.

May 3: APS Cold Fusion Seminar, Baltimore....physicists attack cold fusion claims.

May 5: Raster scanning electron microscope elemental analysis of a piece of Pd from our #2 cell, by the Applied Electrochemistry Group (Dr. Wabner), shows over-

whelmingly the presence of palladium, followed by trace calcium, and minute amounts of copper, and iron. Tiny particles consisting of potassium, chlorine, and of platinum could also be found. The platinum-black evidently came from the anode in the undivided cell. Lithium could not be seen by this technique, as its x-ray energy is too low. However, one could see the lithium by putting the Pd piece in a Bunsen burner flame, and noting its characteristic color.

May 23-25: Workshop on Cold Fusion Phenomena, Santa Fe, New Mexico, sponsored by Los Alamos National Laboratory. We submit a long abstract[30], but are personally unable to attend. Computer summaries become available on the network, and we later obtain video tapes of the entire conference. F&P do not even submit a paper, and the previously touted LANL/Utah collaboration is still frustrated by lawyers. Over 120 papers are presented, the vast majority having "null" results.

May 25: Initial reports at Santa Fe Cold Fusion Workshop, from Texas A& M [31] of Tritium levels in their experiments 10^4 times higher than we have seen in ours, require close scrutiny into their stock D_2O feed supply, or any other sources of Tritium contamination. Also, as they know, should they ever repeat these measurements, they should try it on samples taken before the beginning of the experiment. Texas A& M shows interesting microcalorimetry, claiming to see "excess heat" in the realm of F&P.

We have visited several laboratories, on two continents, and had phone conversations twice directly with M. Fleischmann. Members of our team have had friendly visits with the "cold fusion experiments" at Harwell, Frascati, Karlsruhe, Texas A&M, BYU, and Los Alamos, but were denied access to Pon's lab in Utah. We were also at two meetings of Euratom, the first in Brussels, and the second in Harwell, on the topic of European cold fusion efforts.

B

Sante Fe Workshop on Cold Fusion Phenomena, May 23-25, 1989

Electrochemical "Cold Nuclear Fusion" Attempts at IPP

G. A. Wurden*, H. S. Bosch, J. Gernhardt, G. Janeschitz, F. Karger, J. Perchermeier

Max Planck Institut für Plasma Physik
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Abstract

Following the report of Pons and Fleischmann, we (The Bavarian Bubble Bottle Team) have attempted to reproduce their claims of cold nuclear fusion, and failed. We note that our measurements would not be able to detect neutrons at the level of Jones *et al.* Three experiments were conducted without any signs of neutrons, tritium or gammas above backgrounds, and within ±5% accuracy calorimetry, no excess heating. Intrinsic tritium, differing from each D_2O bottle tested, was however observed.

The third, and most significant attempt used a 22 gram, 10 cm^2, cast (vacuum melted by an e-beam) palladium piece, which was electrolytically loaded with deuterium (99.75% purity D_2O, 0.11 M LiD, Pt mesh anode) at current densities of 200-250 mA/cm^2 for a period of 21 days. Current, voltage, water bath (well-stirred) and air temperature were monitored continuously with a strip chart recorder. The fully covered 170 ml central cell did not partition the electrolysis products, all calorimetry was done in steady state, (bath time constant 100 minutes), and air flow across the 1 liter water bath was kept constant. No isolation Dewar was used. Measurements of the actual temperature(s) directly in the center cell showed strong thermal gradients ($3-4°C$), so calorimetric measurements are only relevant for the well-stirred water bath/air temperatures (typically 45 and 26°C, respectively). An absolute resolution of better than 200 mW, out of 20 Watts typical input power, was obtained. The equilibrium temperature rise of the water bath was strictly linear with input power, calculated from $P_{in} = I*(V-1.54v)$. A Kapton encapsulated 40 Ohm strip resistor was used in the water bath for reference ohmic input measurements. Fast reductions in current density, after waiting for a new thermal equilibrium, yielded nothing unusual. A BF_3 thermal neutron counter, (calibrated efficiency of 1 count/370 neutrons from the same location as the cell, backgrounds of 0.05 counts/sec, with 12 cm polyethylene moderator and a Cd shield), as well as a moderated Li^6I neutron detector, a large high-resolution, high efficiency (170 cm^3) GeLi gamma detector, a 2" NaI detector, and a proportional counter were used to look for radiation. Gamma backgrounds from K^{40}, Bi^{214} and Tl^{208} (Thorium decay in concrete), were easily seen in pulse height spectra. Backgrounds at our second floor location were 120 mrem/year gammas, and 10 mrem/year thermal neutrons, as measured by a Berthold LB1026 Radiation Monitoring system. No special shielding precautions against cosmic rays were used. A 4 Megawatt swimming pool nuclear reactor, 600 meters away from our building, was one of our additional background considerations! Tritium was measured in the electrolyte, D_2O samples, and H_2O (both with and without the LiD solute). A model 2260XL Tri-Carb Liquid Scintillation Analyzer by Canberra-Packard was used, with the old ($\geq$ 15 years) Merck heavy water yielding 210 dpm/ml from one vial, and 9.9 dpm/ml from another used for refilling. By contrast the LiD dissolved in pure H_2O (unneutralized) gave 6.6 dpm/ml. The cell was replenished with D_2O at a rate averaging 16 ml/day. The electrolyte after 19 days of operation measured 150 dpm/ml. In comparison, the D_2O from another bottle (with same Merck #, and purchased at the same time!) used in our first 14 hour experiment had 750 dpm/ml. Pulse height analysis suggests that true Tritium decay signals are present, and chemical fluorescence in the "cocktail" was not important (although measurable). Tritium can reasonably be explained from that originally present in the various D_2O flasks.

The experiment was terminated on April 28, by throwing the loaded palladium sample directly into liquid Nitrogen, immediately next to the bare BF_3 counter (backed by 25 cm of moderator), in order to attempt one of the Italian ENEA neutron production variants. No neutrons (sensitivity of 5 n/sec equivalent source strength) above backgrounds were seen, while counting for one hour, and also none while the piece warmed to room temperature over the next hour. Post mortem analysis of the darkened, hardened Pd piece showed large crystal grains (up to 2mm×2mm), and continuing evolution of gas bubbles at the grain boundaries even four days after the experiment was ended.

* Los Alamos National Laboratory, presently at ASDEX, supported by USDOE

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DOE/S-0071

Interim Report of the Cold Fusion Panel of the Energy Research Advisory Board

August 1989

A Report of the
Energy Research Advisory Board
to the
United States Department of Energy

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DOE/S-0071

Interim Report of the Cold Fusion Panel of the Energy Research Advisory Board

August 1989

A Report of the
Energy Research Advisory Board
to the
United States Department of Energy
Washington, DC 20585

INTERIM REPORT

OF THE

COLD FUSION PANEL

OF THE

ENERGY RESEARCH ADVISORY BOARD

AUGUST 1989

U.S. DEPARTMENT OF ENERGY

Energy Research Advisory Board
to the
United States Department of Energy
1000 Independence Avenue, S.W.
Washington, D.C. 20585
(202) 586-5444

August 17, 1989

Admiral James D. Watkins
The Secretary of Energy
US Department of Energy
1000 Independence Avenue, SW
Washington, DC 20585

Dear Admiral Watkins:

It is my pleasure to send you the interim report of the Cold Fusion Panel of the Energy Research Advisory Board. The report has been approved by the full Board by letter ballot.

The Panel has worked diligently to respond to your charge and the members deserve a great deal of credit for the dedication and expertise they have brought to this assignment.

The Board concurs with the Panel's overall finding that cold fusion does not merit any special program or major expenditures at this time, although a modest level of funding would be appropriate to resolve outstanding issues as identified in the report.

The Panel consists of 22 distinguished scientists, including chemists, physicists and metallurgists. In reviewing the research on cold fusion, the members visited a number of laboratories involved in this effort, including the University of Utah, Brigham Young University, Texas A&M, Stanford, and the California Institute of Technology. The Panel has also been in contact with prominent independent scientists in the United States and abroad who are engaged in this or related research.

We hope this interim report will be useful to you. We plan to submit a final report in November.

Sincerely,

John Landis
Vice Chairman

Attachment

UNIVERSITY OF
ROCHESTER

COLLEGE OF ARTS AND SCIENCE
DEPARTMENT OF CHEMISTRY

John R. Huizenga
Tracy H. Harris Professor

July 20, 1989

Mr. John Schoettler
1580 Lincoln Street, Suite 1200
Denver, CO 80202

Dear John:

I am pleased to forward to you the Interim Report of the Cold Fusion Panel. This report reviews the current status of cold fusion and makes some preliminary conclusions and recommendations, as requested by the Secretary of Energy.

The Panel or subgroups thereof have participated in the Workshop on Cold Fusion in Sante Fe, have visited several laboratories, have studied the open literature and numerous privately distributed reports and have participated in many discussions. In addition, the Panel held three public meetings where its findings were discussed and drafts of the Interim Report were formulated.

I wish to thank the members of the Panel and its staff for their cooperation and their diligent work during these weeks. The Panel anticipates that their final report will be completed in November, 1989.

Sincerely,

John

John R. Huizenga
Co-Chairman,
Panel on Cold Fusion

467 Hutchison Hall
University of Rochester
Rochester, New York 14627
(716) 275-4217

INTERIM REPORT OF THE COLD FUSION PANEL OF

THE ENERGY RESEARCH ADVISORY BOARD

INTRODUCTION

As a result of the startling announcements in March 1989 by Utah scientists claiming the attainment of cold fusion, the Secretary of Energy requested (see Appendix A) that the Energy Research Advisory Board (ERAB) convene a panel (see Appendix B) to assess the possibility of cold fusion. The panel meetings and schedule of laboratory visits are summarized in Appendix C.

Since the above announcement, many laboratories worldwide have initiated research in cold fusion. In the United States, a major effort has been undertaken to search for cold fusion by a large number of research groups at industry, university, and national laboratories. Unfortunately, at the present time, the reports from different laboratories are quite divergent. Some laboratories claim excess power production attributed to cold fusion, usually for intermittent periods and for various periods of time, but with no supporting evidence for the production of commensurate quantities of fusion products. Other laboratories find no measurable excess power production and no measurable high levels of fusion products. Some laboratories attribute the discrepancies to inaccuracies in measurements, others to non-reproducibility of a new and not understood process. Tritium levels above normal have been reported in some cells following electrolysis, but not in others. Neutrons near background levels have been reported in some D_2O electrolysis and pressurized D_2 gas experiments, but at levels 10^{12} below the amounts required to explain the experiments claiming excess power.

Since early May 1989, the Panel or subgroups thereof have participated in the Workshop on Cold Fusion in Santa Fe, have visited the laboratories listed in Appendix C, have studied the open literature and numerous privately distributed reports, and have participated in many discussions. This report is not concerned with the well established process of muon catalysis, which has sometimes also been called cold fusion.

GENERAL CONCLUSIONS

Although the Panel's task is not yet completed, the Panel finds that the experiments reported to date do not present convincing evidence that useful sources of energy will result from the phenomena attributed to cold fusion. Indeed, evidence for the discovery of a new nuclear process termed cold fusion is not persuasive. Hence, no special programs to establish cold fusion research centers or special programs to support new efforts to find cold fusion are justified at the present time.

However, there remain unresolved issues and scientifically interesting questions stemming from reported cold fusion efforts. Some of these are relevant to the mission of DOE and should be handled by carefully focused and cooperative efforts within current programs by normal mechanisms for project selection.

The reports of excess heat and fusion products are assessed in separate sections. Preliminary recommendations are summarized in the final section.

CALORIMETRY AND EXCESS HEAT

The claim for electrochemically charged palladium cells as prospective energy sources rests on reports of "excess heat" (or, more precisely, excess power) that cannot be accounted for in the thermal balance normally applied to water electrolysis. Among the issues the Panel addressed in site visits were whether the power levels themselves are being accurately measured and whether the reactions being considered in these cells are, in fact, satisfying the chemical assumptions made. These heat measurements have been done with calorimetry that varied as to technique and to levels of precision and accuracy. In most cases, calorimetric effects attributable to excess heat are very small. The calorimetric measurements are difficult to make and are subject to subtle errors arising from various experimental problems.

For the purposes of this report, the calorimetry is usefully differentiated as to whether the D_2 and O_2 gases are allowed to exit the cell completely unreacted or are intentionally catalytically recombined to regenerate D_2O and to recover the corresponding heat. In the case of open cells, where the gases are assumed to be vented without reaction, any output power (as heat) greater than the electrical input power minus the power equivalent of the D_2O formation enthalpy [1.527 V(volts) x I (cell current)] is considered excess, a result reported by several groups. In closed cells with total recombination (and with a deuterium-charged Pd electrode), the total electrical power in and total heat power out would normally balance (as for Pt and Pd electrodes in light water). At present no experimenters who have performed calorimetry with closed cells under strict recombination conditions have reported any excess heat. Another important point is that most of the reported measurements with open cells are actually power measurements, and the data have not conclusively demonstrated that the total amount of energy produced (as heat and chemical energy) exceeds the total electrical energy input.

Since the claimed excess heats have, in most cases, been of a magnitude significantly less than the 1.527 V x I factor itself, issues of calibration, reliability, and support of the assumptions of zero recombination are especially critical. The Panel's site visits have identified experimental uncertainties, e.g., nonlinearities of the calibration in power output vs. temperature, time dependence of calibration, and doubtful accuracy of data acquisition relative to the magnitude of the effects asserted. Even in laboratories that

report excess heat, this effect, under apparently identical conditions, is not reproducible. In none of our visits to the different sites did we see an operating cell that was actually producing excess heat. So far, we have seen no experimental results that are sufficiently free of ambiguities and calibration problems to make us confident that the steady production of excess heat has been observed. However, there are reports of sporadic temperature "excursions" or "bursts" that apparently represent power outputs significantly larger than the input power. These events cannot be attributed to problems with accuracy or calibration alone and are presently not understood. In general, the calorimetry to date does not persuasively demonstrate the production of excess heat, but the bursts will require evaluation in the Panel's final report.

FUSION PRODUCTS

Since deuterium fusion necessarily yields fusion products (neutrons, protons, tritium, ^{3}He, ^{4}He, gamma rays), it is essential to establish the presence of such products in any claim of fusion. Each watt of power must be accompanied by about 10^{12} particles per second. This makes product detection by far the most sensitive method to search for fusion. Results to date on fusion products are summarized in the following paragraphs.

Neutrons are an established signature of the well studied d+d fusion reaction. Although many experimenters report no neutrons, some report as many as
0.1 neutron per second. If confirmed, this rate would be of scientific interest (even if not indicative of cold fusion). This rate is so far below the 10^{12} neutrons per second required for 1 watt of power generation that it is of no interest as a practical energy source.

Numerous experimenters have sought tritium production in electrochemical cells and have found no excess tritium. One group reports finding up to 10^{14} tritium atoms (neglecting losses to the gas phase) in each of several cells with Pd cathodes and Ni anodes. Some of the same experimenters report neutrons produced from similar electrochemical cells, but at a rate of only about 0.1 neutron per second. If the tritium were a result of deuterium fusion, the rate of neutron production should be comparable, and thus some 10^{11} times greater than reported.

Another important fusion signature is ^{3}He, which should be detectable within a cathode after operation at fusion power levels of watts. It has been postulated that the cold fusion reaction might conceivably proceed predominantly by the production of ^{4}He and thermal energy. None of the researchers to date, including those reporting the production of heat, have reported ^{3}He or ^{4}He above the detectable level of 10^9 atoms. One watt-hour of energy corresponds to more than 10^{15} He atoms.

Low level cold fusion in geologic processes has been proposed to cause high $^{3}He/^{4}He$ ratios and tritium abundances associated with volcanoes. Several laboratories are currently attempting to detect volcanic tritium.

INTERIM RECOMMENDATIONS

1. The Panel recommends that the cold fusion research efforts in the area of heat production focus primarily on confirming or disproving reports of excess heat. Emphasis should be placed on calorimetry with closed systems and total gas recombination, use of alternative calorimetric methods, use of reasonably well characterized materials, exchange of "promising" electrodes between groups, and careful estimation of systematic and random errors. Cooperative experiments are encouraged to resolve some of the claims and counterclaims in calorimetry. Such experiments should be pursued at a limited number of laboratories and supported at a modest level on the basis of competitive proposals. At the present time, the panel recommends against any significant expenditures to establish cold fusion research centers or to support new efforts to find cold fusion.

2. A shortcoming of most experiments reporting excess heat is that they are not accompanied in the same cell by simultaneous monitoring for the equivalent production of fusion products. If the excess heat is to be attributed to fusion, such a claim should be supported by measurements of fusion products at commensurate levels.

3. Experiments designed to check the reported production of excess tritium in electrolytic cells are desirable.

4. Experiments reporting fusion products (e.g., neutrons) at a very low level, if confirmed, are of scientific interest but have no apparent current application to the production of useful energy. Continued support of such experiments at modest levels is justified, provided the proposals for such research are evaluated in comparison with other DOE research proposals. In view of the difficulty of these experiments, collaborative efforts are encouraged to maximize the detection efficiencies and to minimize the background.

APPENDIX A

The Secretary of Energy
Washington, DC 20585

April 24, 1989

Mr. John H. Schoettler
Chairman
Energy Research Advisory Board
US Department of Energy
1000 Independence Avenue, SW
Washington, DC 20585

Dear Mr. Schoettler:

In recent weeks, there has been a great deal of interest in the prospects for "cold fusion", based on experiments at universities in Utah and subsequent experiments performed elsewhere. At present, the apparent observations of cold fusion and significant quantities of energy from this phenomena are being investigated extensively. Because of the potential benefits from practical fusion energy, I request that the Energy Research Advisory Board (ERAB) assess this new area of research. Specifically, I would like the Board to:

1. Review the experiments and theory of the recent work on cold fusion.

2. Identify research that should be undertaken to determine, if possible, what physical, chemical, or other processes may be involved.

3. Finally, identify what R&D direction the DOE should pursue to fully understand these phenomena and develop the information that could lead to their practical application.

I request that the Board provide an interim report on the first item by July 31 and a final report on all items by November 15, 1989.

Sincerely,

James D. Watkins
Admiral, U.S. Navy (Retired)

ENERGY RESEARCH ADVISORY BOARD
COLD FUSION PANEL

6/02/89

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Tracy H. Harris Professor of
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APPENDIX C

PANEL MEETINGS AND SCHEDULE OF LABORATORY VISITS

PANEL MEETINGS

Washington, DC	June 22, 1989
Washington, DC	July 11-12, 1989

SCHEDULE OF LABORATORY VISITS

University of Utah	June 2, 1989
Brigham Young University	June 13, 1989
Texas A&M University	June 19, 1989
California Institute of Technology	June 20, 1989
Stanford University	July 6, 1989
SRI International	July 6, 1989

OTHER

Workshop on Cold Fusion, Santa Fe, NM	May 23-25, 1989